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OF

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ARTICLE I.

THE ANNUAL DISCOURSE.

THE PRESENT STATUS OF THE PRACTICE OF MEDICINE, AND THE URGENT NEED OF MORE STRINGENT APPLICATION OF HYGIENIC AND SANITARY LAWS.

By WILLARD S. EVERETT, M.D.
OF HYDE PARK.

DELIVERED JUNE 11, 1902.

NOTE.—At an Adjourned Meeting of The Massachusetts Medical Society, held Oct. 3, 1860, it was

Resolved, "That The Massachusetts Medical Society hereby declares that it does not consider itself as having endorsed or censured the opinions in former published Annual Discourses, nor will it hold itself responsible for any opinions or sentiments advanced in any future similar discourses."

Resolved, "That the Committee on Publications be directed to print a statement to that effect at the commencement of each Annual Discourse which may hereafter be published."

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THE PRESENT STATUS OF THE PRACTICE OF MEDICINE, AND THE URGENT NEED OF MORE STRINGENT APPLICATION OF HYGIENIC AND SANITARY LAWS.

MR. PRESIDENT AND FELLOWS
OF THE MASSACHUSETTS MEDICAL SOCIETY:—

Such is our theme to-day. But you must not expect to listen to a scientific exposition, or an elucidation of some new or great principle or application of a principle, that is destined to overthrow prevailing customs or change the practices of men. You must not prepare yourselves to find a new or unexplored mine of intellectual wealth revealed. For these acquirements search must be made elsewhere.

But happily an exhibition of great learning is not needed now. The Shattuck lecture, with its marvelous wealth of scientific instruction, and with the exhaustive treatment of the subject, so ably presented by our honored guest; the various dissertations that have been given in the different sections, together with the comments and discussions by which they have been followed, have supplied all the intellectual and scientific aliment that is needed to redeem the exercises of these anniversary days from the charge of being useless and unprofitable, and this one hour may well be devoted to lighter themes.

And so I will invite you to be patient with me while I shall indulge in some of the reflections, and make some of the suggestions, and state some of the conclusions that have been evolved from the experiences and in the course of a practice extending over a period of nigh forty years. And

as the events and transactions of those forty years pass in review before us like the shifting scenes of a panoramic landscape, memory will be busy with its visions and thought gravitates easily and naturally into meditative and reminiscent moods. And it is but the natural exercise of the ordinary functions of mental activity that leads to a general survey of the whole broad field that lies before us, and attempts to penetrate, even with our imperfect vision, some of the mysteries that lie beyond.

The changes of those years have been many. The men who were then in active life, and who were then giving character to the medical profession, are gone. But few of those who were in active practice when those forty years began, are with us now. New customs have supplanted the old. New practices have displaced those that prevailed at that time. New measures have been introduced. Former ones have become obsolete, and with the men that sustained them are gone to the shades. Altogether the former things have passed away. Changes are apparent everywhere, and but little remains except the memory of those fleeting but eventful years. But medical science has not stood still. Increased facilities for acquiring knowledge, improved instrumental aids, and industry and devotion have produced their results.

In some lines of human endeavor the advancement and the progress of medical science have been magnificent and great. In the department of surgery, and in all that pertains to the surgical art, its achievements and its victories have outrivaled the dreams of visionaries of a few years ago. To us, from whom the future is hidden, it almost seems as though the limit had about been reached.

It is not easy for one endowed with only common faculties of observation and reason, to imagine in what direction improvement in surgical processes can much farther go. Yet others have thought so before our time. Ambrose Paré

thought so once, and informed the world in 1579 that there seemed to be but little left for posterity to accomplish "but a certain small hope to add some things." His words sound to us like boasting. But the man who sent the torturing searing iron, the hot pincers and the blazing pitch to the museums of the antiquaries, to rest there forever as surgi cal appliances and as curiosities of the days gone by, by substituting for them the simple ligature for the arrest of hemorrhages, may well be pardoned for some self-glorification, for the changes he himself had wrought in surgical processes, though there yet remained such marvels of progression to be revealed in the future, that then were hidden from his dazzled gaze. We wonder, as a clearer perception of his position dawns upon us, if the medical antiquary of three and a half centuries hence, as he delves into the archives of our epoch, will indulge in the smile of complacency or of derision, with which we read his words.

And we wonder also what can happen that shall make the chasm between our own times and the centuries that are coming, as wide as that which separates us from the times of which he wrote. Yet we know not what a day may bring forth. Surprises in surgery are constantly occurring, and it may be confidently predicted that in some way or other a progressive future awaits the art of surgery yet.

In the department of medicine, however, though the changes have been many, if it is true that by their fruits ye shall know them, we cannot feel quite the same assurance that the real advances in dealing with diseases have been correspondingly great. Yet we do not forget the work that medical science has done. We do not forget Jenner, nor Pasteur, nor Koch, nor those who follow in the paths where they have led. We do not forget that great learning, immense wealth and untiring industry are laid upon its altar, and are expended without stint or limit in the study of diseases, and we are not unmindful of the result.

We do not forget the means that have been perfected for our assistance, nor the aids that have been placed in our hands. We do not forget all that we owe to the clinical thermometer, the use of which had never been suggested fifty, and scarcely forty years ago. We do not forget all that has been learned from urinary analyses, that were rarely thought to be essential then.

We do not forget the hypodermic needle, nor how opportunely or providentially the antitoxins have come to our aid. We do not forget the thyroid extract, by means of which the unsightly and distressing myxedema has been made to yield. We do not forget what the Röntgen rays are doing in making plain what was hitherto invisible, nor that their palliative, if not curative, action upon sarcoma and carcinoma and epithelioma are rapidly passing beyond the experimental stage.

We do not forget the improvement that followed when the indiscriminate use of the lancet yielded its place to milder measures or to gentler stimulation, nor the disgust that was avoided when the nauseating doses of a former period were so largely superseded by the triturate tablet and the sugar or gelatine coated pill.

And we do not forget the wonderful improvement in pharmaceutical preparations, that the progress of the times, as directed by medical science, has demanded, and that these latter days have ushered in; and contrasting them with the vile concoctions that were forced upon the sick and the suffering, in the days when crabs' eyes and blind puppies, earth worms and human skulls, had a place in recognized formulæ, in standard pharmacopeias of the period, and a commercial value as staple articles in the catalogue of pharmaceutical supplies, we felicitate ourselves with great joy and gladness that the lines have fallen to us in more pleasant places, and bless with sincerest gratitude our ancestral lineage that our existence was delayed to these later days.

And finally, we do not forget the great multitude of investigators that have so long been focussing their microscopic lenses upon every form of pathological condition, with the hope and determination of tracing the ultimate causes of every form of disease.

We do not forget that never in the history of medicine or of the world have the searchlights of science been cast more persistently or more determinedly upon these bewildering problems, to work out their solution, than in these later years.

Never have clearer heads or keener intellects or braver hearts grappled with these perplexing difficulties, either to find the way to overcome them or to demonstrate the impracticability of the attempt. Indeed, so far has science already led us, that the personal element in the profession is being largely eliminated from the account, and the oldtime independent thought, self-reliant judgment and prompt, decisive action — not always without their errors, it must be admitted, yet useful in many ways — that belonged to a former generation, are rapidly and it is to be feared altogether, disappearing; and where once the way seemed clear before us, and pathognomonic symptoms clearly and sharply defined; we now hesitate and falter, if we do not positively decline — even while disease is rushing onward with increasing and dangerous rapidity — to render a diagnosis, until the revelations of the microscope or the bouillon cup have either exploded or sustained the views we have entertained. And we do not question their conclusions. We only learn to distrust our own.

And if there are those among the older practitioners who ask if the best interests of the sick and the dying are always subserved in this way; whether the accuracy of a mathematical demonstration is always secured by methods like these; if there are those who claim that habits of lifelong observation and years of experience at the bedside are en-

titled to some consideration still; if there are those who believe that the trained perception, the quick ear, the keen eye, the sensitive touch, that years of practice have intensified, if they have not perfected, are entitled to a place for practical purposes in the armamentarium of the physician that neither the exhibitions of the microscope nor the logic of the laboratory can fully supply, and that they will sometimes lead to just conclusions, before either the microscope or the laboratory can so prepare and adjust their specimens as to pronounce definitely upon the result; yet even these are willing to concede, when they do not conceal their misgivings, and while reserving to themselves the privileges of private judgment and independent action, do not hesitate to fall into line in the great procession that marches to the music of progression where the heralds of science blaze the way. And it is well. Heaven grant to these pioneers of science an unobstructed sway! Heaven grant to them abundant success to their labors! Heaven grant that their work may continue to prosper, until they shall find not only the germs of all diseases, but shall also discover the means that shall dislodge them from their place!

All honor to those noble men. All honor to those worthy ones who are denying themselves so many of the things that add to life's enjoyment, that they may devote their best years to this beneficent work. All honor to those who are teaching us the beginnings of disease. Let all encouragement be given them to continue in the paths they are pursuing, till they shall also teach us how to bring their work to a successful end. Let all praise be theirs for results already won. We honor them for their devotion to the work they have undertaken; we respect their conclusions; we follow their directions; we adopt their recommendations; we proclaim their fame while they are living, and rear monuments to commemorate and prolong their memories when they are gathered to their rest.

But the old conviction comes back to us still. If the purpose of study and of treatment is to cure disease, and so to promote the welfare of humanity, then the ultimate purpose of medicine is not yet in sight. When all our investigations are completed, when our studies and researches are done, when the improvements in management and the additions to our resources have either demonstrated their value or failed in our hands, the grand object of curing disease by medication, so far at least as many diseases are concerned, is not gained.

Tuberculosis remains the same unconquered and unconquerable foe to human life that it has been always. And in cases where improvement has been noted, it has been accomplished less by medication than by hygienic means. Pneumonia swept away 5,282 victims in 1900, in Massachusetts alone. Typhoid fever remains typhoid fever still. And neither improved—or at least varied—methods of treatment, nor cold ablutions, nor chilling immersions, nor Widal tests, nor microscopic views, nor corpuscular computations, have perceptibly changed its character in any essential particulars, from the fatal and fearful affection that baffled the skill of the physician in the days of our predecessors, and that resists with equally stubborn obstinacy the best directed efforts of our own. It regards neither rank nor condition; it abates not one degree of its destructive energy, whether its visitation fall upon the heedless victim of his own carelessness and needless exposure, or on our own cherished and beloved governor, who relinquishes the chair of State and retires from the duties of the gubernatorial office long enough to come in here with us year after year, to honor us with his presence, and to speak to us words of encouragement and praise.

And the humiliating thought that adds the keenest pang to our affliction is that both the sanitarian and the medical profession are committed to the opinion that its place is in the

catalogue of preventable disease. There were fifteen diseases whose fatality was greater in Massachusetts, in both 1890 and in 1899. Whether its rank throughout the country in those years was higher we do not know. But in 1896, Dr. Bradford tells us, in the Annual Address* for 1899, that it claimed 75,000 victims in the United States.

We know not and are not now considering "whether disease is a part of the plan of creation," but we know that existence is hedged round with so many unfavorable conditions, that danger surrounds us like an atmosphere from life's beginning to its close. In whatsoever paths our feet are guided, disease is everywhere present with us still.

We do all that medical science in the present state of its development suggests and all that experience teaches, to counteract the ills that beset us, that we are obliged to acknowledge we cannot prevent at present and do not always cure. We build hospitals in every city, and furnish them with every convenience and necessity for the care of the helpless and the sick. We secure to them the best attendance that the medical profession can supply. We build expensive laboratories, and equip them in liberal fashion with every useful appliance, in the hope that every new aspect as it makes its appearance, as well as every structural change, may be noted and assigned to its appropriate place in the etiology of disease. We exhaust our knowledge of the pharmacopeia and the products of the earth, the sea and the atmosphere in devising new antidotes for the relief of whatsoever maladies afflict the children of men. We experiment with every new remedy or combination of remedies that is commended to our attention as possessing special adaptation to special diseases or uses, to the end that we may thoroughly test their virtues, and when they have disappointed us sufficiently we cast them aside. We invoke

* Medical Communications, Massachusetts Medical Society, vol. xviii, No. 1, 1899, Annual Address, p. 33.

the aid of all the resources that can be made available or be supplied from any quarter, to counteract the ravages of those living organisms that science tells us are wasting life away. We dismember the body when its life has departed that we may subject its every tissue to a scrutiny that was impossible while its subject was living, to ascertain the ultimate structure as well as function of the minutest filament of muscular fibre or the smallest spiculæ of bone. Neither nerve tissue nor brain substance escapes us. Nothing is overlooked, and nothing satisfies until we have resolved the whole human structure to Virchow's universal cell, and located all our many troubles in the cellular transformations and atomic changes that have their origin there.

We stifle our feelings of humanity and pity, in the interest of science, and subject the lower orders of creation to direst suffering and agonizing death, and composedly study from their vicarious anguish the pathologic processes that, artificially induced in their less respected bodies, seem to be identical with the cellular changes that take place in the human system in the same disease, and in so doing, hope to learn from their distresses how to relieve our own.

We try to follow in the paths wherein the men of science lead us, though we may not fully comprehend their ways. We study the published reports of their proceedings, with the determination—if not with an entirely successful attempt—to master and appropriate to our own uses the lessons of wisdom that they teach us, and make in some measure their wealth of scientific lore our own.

We apply ourselves with diligence and assiduity to the mastery of the marvelous vocabulary and nomenclature that has been developed in the progress of the science, and tax our mental machinery to its limit, while we wrestle with the intricacies and technicalities of accurate scientific expression, till microbe and leucocyte, bacteria and bacillus, and spherical cell, and cylindrical cell, and spindle cell, and miccro-

coccus, and streptococcus, and staphylococcus, and diplococcus, and spores, and ptomaines, and spirillum, and vibriones, become familiar, if not always distinctly differentiated or perfectly understood, terms.

We divide ourselves into schools and systems and specialties, and argue and dispute with one another in controversial earnestness, even to the verge of desperation and ill temper, in vainly attempting to discover or determine which modes of procedure of all that are presented are best—for the patient's well being, if so it may be—or, failing that, for bringing to our own exchequer the amplest return.

We do all this, and we do much more. We hold ourselves in readiness to respond to every call for medical assistance, however slight the occasion or attended with whatsoever of inconvenience or self-sacrifice it may be.

We sink into the depths of despondency and discouragement when death defeats our best endeavors, and hardly recover from our disappointment to a normal plane of hope and cheerfulness when—as does sometimes happen—we have the satisfaction of believing that we have been instrumental in prolonging a life. We grapple courageously and fearlessly with diseases of every description, however dangerous in their character, as though there were neither contagion nor infection nor personal danger in the world.

We do all these things. We fight to the last to save our patients, but the arch enemy is mightier than we. Disease and death will not be vanquished. They remain with us still. The fight with them goes on forever. It has been going on through all the centuries. It is going on still. The combat is never ended. The battle is never won. Our efforts to overcome them are thus far largely futile. Our most earnest endeavors to eradicate them are thus far at least partially in vain.

Men die—die in the vigor of useful and prosperous manhood; die when life's responsibilities are greatest; die

when ties of affection are strongest; die when life seems most essential to the family and to the world. Youth and beauty are stricken by the fell destroyer, and go down to the silent chamber, ere yet the first taste of life has lost its exhilarating flavor, and fade out of existence ere yet the mildew of time has touched the hem of the garments that adorned their primal bloom. Fathers and mothers stand in hopeless but unavailing sorrow round the couch where the pride of their eyes and the joy of their hearts is breathing out its infantile and agonized existence, and gaze imploringly, beseechingly — and it may be reproachfully — into the face of the physician, whose countenance does not cheer them, whose word can yield them neither hope nor encouragement, and whose impotent hands are powerless to bring them relief. There is no need to overdraw the picture; but it is useless to keep the truth from sight.

Statistics might reinforce these statements, but everyone knows that they are true. Or if there are those who doubt them, let them search the registration reports, and review their own experiences for themselves. We are assured that the death-rate is diminishing, and gladly believe that it is true. But granting all that is claimed for increasing longevity, the rate is yet sufficiently high. The churchyards are constantly receiving their accessions, are multiplying in numbers everywhere, and are extending their areas; and because they are encroaching too largely on valuable territory, as well as becoming in themselves a menace, the crematory fires are kindled and are blazing with fervent heat.

And this deplorable mortality is not confined to the aged, whom all know must die. Neither can it be attributed to a scarcity of physicians, for there are but eight states in all our broad land where there are 1,000 persons to each physician; and from thence the range is down to one in 420 in California, which seems of all the states in the Union, to be most abundantly supplied. In Massachusetts, in whose

welfare we are more particularly interested, the ratio is one to every 539. Throughout the whole country there is, or was, in 1898, one registered physician to every 647 persons. And yet this excessive mortality exists, and it is not an accident of recent times. It is as ancient as the historic page. It was thus in the days of the psalmist. It was thus when the poet paraphrased his words:

> "'Tis but a few whose days amount
> To threescore years and ten."

It was true when these words were written, and it is true now. Disease is lurking everywhere. Life is imperilled at every step. The causes that induce the one and sustain the other seem mingled in unutterable and inextricable confusion, and overwhelm us in doubt and perplexity, from which while existing conditions continue, we can look forward to no release.

The atmosphere that supports respiration is laden with pathogenic germs. The elements that supply nutrition become the messengers of death. The mother's kiss imparts contagion, and the hand that is grasped in the fervor of friendship finds contamination concealed in its clasp. The balmy Southern breezes that refresh and cool the fevered forehead waft their miasm from the savannas of the southland, and the rugged northern tempest that imparts its energizing vigor brings its microbe with its chill. It was a microbe that sapped out the lifeblood of the blue-eyed and fair-haired creature that grew up with you in your boyhood, whose hand restrained you in the recklessness of adolescence, when even your mother's wishes were not always obeyed, and guided you into paths that have led to your present high estate, and whose loss left a void in your existence that the world has never filled, when she died; and it was a microbe also that caused the bread she ate to rise.

The same organisms that produce the ferment that keeps the earth in condition suitable for human habitation, are

more destructive to those who dwell upon its surface than the rifle or the sword.

The insect world arrays itself with the forces that compass our destruction, and comes swarming round us in its myriad forms, not alone destroying our comfort with its annoying presence, but exposing us to dangers that are too subtile for our unsuspecting senses to recognize, but too powerful also, in their aggregate numbers, to be easily dispelled or controlled. And even the contemptible and villainous little mosquito, seemingly too insignificant to be worthy of notice, and which we feel it to be beneath the dignity of manhood to recognize, even as an annoyance — if it is the truth that is told about him — has sins and transgressions little less diabolical than murder, laid to his charge. Some one may object to the use of the masculine pronoun here, because they tell us that it is the female, in this instance, that makes the mischief, as it was at creation's birth. But perhaps the generic title may be permitted here as being adapted to our present purpose, while we leave them to arrange the question of individual responsibility between themselves. Yet against the insidious attacks and incursions even of so despicable an enemy, we have neither defense nor protection, save only by calling to our relief, assistance, and even preservation, the whole gigantic machinery of the Standard Oil Trust, which backed by all the Rockefeller millions, is able to furnish only a partial guarantee of immunity, in the ratio of fifteen square feet to the ounce. In this new David and Goliah contest the advantage is certainly again with the sling. Yet life has greater foes than he; for when the worst has been said against him, it must be admitted that at worst he is an open enemy. Whatever potentialities of mischief may be concealed within his diminutive figure, his attacks are never disguised. We can see him, we can hear him, we can feel his sting; and warned in this manner of his presence, we are given some opportunity to protect ourselves.

But the greatest foes of existence are those that do their work unseen and in silence, and that nothing less powerful than a microscope can render visible to human sight. Yet the devotees of science assure us that in these microscopic organisms are traced the elementary sources of all disease; and they assure us, also, that it is to counteract or prevent their ravages that the best energies of medical science can be most effectually applied.

Such, then, appears to be the status of medicine, as these first years of the century begin. We note many improvements, we recognize many and great advances in the administration of medicines, but we cast our gaze outward over a world of sickness and suffering yet. The normal condition of existence should be one of perfect health. The perfect type of life on this earth is a sane mind in a sound body, from the earliest moment of existence, through the intermediate stages of development and maturity, until it shall terminate finally in the processes of natural decay. Yet a condition of perfect health in any large community is nowhere to be found. Some allowance may perhaps be charged to the inherent imperfections of our nature. But if we attempt to account for the universal prevalence of disease upon any such theory, we deceive ourselves.

Do we find encouragement and comfort in comparing our own times with the past? We search through its annals, as guided by the light of history, to find that in the sixteenth century the average duration of human life was from eighteen to twenty years. At the beginning of the nineteenth it had reached to thirty years. And now, as the twentieth century opens, it is said to have been extended beyond forty years. But why tell of that? Why count that as a victory for therapeutics which possibly may have been accomplished in other ways? Are we certain that the administration of medicine has produced the change?

Running parallel with all this increasing longevity, and

precisely corresponding with it, both in regard to times and localities, are better modes of living, fewer unseemly and unwholesome practices, wider streets, purer air, and the introduction of sewerage systems and unpolluted water supply — all advances in sanitary and hygienic conditions, with which therapeutic agencies have nothing to do.

And what of the future now? Signs are not wanting that indicate along what lines the advances of the twentieth century shall come. And prevention seems, in the light of history, more promising in its results than attempts at cure. Probably hygienic measures will never supersede the use of therapeutic remedies in the treatment of disease. But we well may anticipate with satisfaction the approach of that season when they shall be blended together in a closer and more beneficent union than the world has ever yet known.

Preventive medicine is no new thing. Nor is it one whose importance or influence upon the public health need be urged upon a body of physicians for the novelty of the subject or for the originality of the thought. Its importance has long been recognized. Boards of health have been appointed specially to enforce its requirements. Volumes have been written upon public hygiene. Preventive medicine and the discussions that have followed its recommendations have long occupied a prominent position in the medical literature of the times. Discourses have been read to us that have placed the subject before us in a manner as convincing and as truthful as can find expression in language, and in terms that admit of no reply.

Boards of health, both state and local, have urged upon us the need of improvement in sanitary conditions, with force of statement and cogency of reasoning that no argument can refute. Public health associations make sanitary science their chief purpose and study. National conferences of state and provincial boards of health seek to secure cooperation and uniformity of methods of doing their work.

The American Medical Association organizes a section to discuss state medicine and public hygiene. The American Climatological Association wrestles with the problems that our ever varying and always trying climate presents. They all tell the same story. They all say: Pure air, pure food, comfortable clothing, freedom from exposure to pathogenic germs.

We have been told also, until the repetition induces a sense of weariness, that wherever putrefactive processes are going on; wherever there are accumulations of that material which we call filth in contradistinction from dirt; wherever stagnant pools or swamps abound; wherever marshes exhale their deleterious vapors; wherever tenements are overcrowded, and wherever lack of proper ventilation exists; wherever food products of every kind are placed upon the markets, and so upon the tables of the hungry, in improper condition; wherever agricultural processes are carried on, especially such as relate to milk production and meat supply, without proper regard to sanitary requirements and purity and cleanliness; wherever the effluvia from sink drains and house offal mingle with and vitiate the air that is breathed; wherever noxious weeds or plants contaminate the atmosphere with their infectious exhalations; wherever drinking water is polluted from any cause; wherever streams and rivers are made foul by waste from manufactories, or from being used as receptacles of decaying matter of every description, all along their courses; wherever the smoke nuisance envelops us like a cloud; wherever these things exist, we have been told sufficiently often that danger exists also. There the microbe revels; there the bacillus watches for his prey. And there preventive medicine and preventive measures are needed always, to destroy their power of doing harm.

It is true that the work of the medical profession appears in all this. It is true that it is science — medical science

— that makes these matters plain. It is true that it was medical science that first discovered the microbe, and it was medical science also that first suggested the measures that should terminate his work. It is true that the guiding hand of medical science has given direction to all these proposed or actual measures of sanitary improvement and reform. It is true that it is because of medical science that the rivers of waters that take their rise among the unpolluted springs and streams of the highlands and the mountains are rushing onward with impetuous speed through their conduits and channels, in haste to allay the thirst of the famishing millions that crowd the cities' contracted areas. It is true that because it is the best method for its disposal that medical science can devise at present, that so much of the waste of the earth that is concomitant with existence is drained through miles of impervious conductors and buried in the sea. It is true that whatever improvement is to be noted in these, and in other particulars, has resulted from medical science and the facts that medical science has made known. It is true that they who have been instrumental in inaugurating all these reformatory enterprises, whose benefits are already so great, as well as in projecting others which it must rest with the future to complete, have been chiefly medical men. It is true that it has been the voice of medical science that has set forth so clearly the dangers that still are threatening, and it is the voice of medical science also that is yet busy in calling for measures of relief. It is true that it has been the trumpet blasts of medical science that have brought these topics to our attention, and have thundered their ominous warnings so often and so loudly in our ears.

But repetition does not stimulate to activity. Argument does not renovate a broken constitution. Discussion does not destroy the industrious germ. And so, in defiance of all that medical science or common knowledge has taught

us, these dangers yet remain — a constant menace to life and health. Our rivers are too often streams of pollution; our tenements are overcrowded; our food products are not always above suspicion; our milk supply is not always free from danger; our drinking water is not always pure. And so there is work yet for medical science to do.

The intense activities that have marked the nineteenth century have gone far in many beneficent directions, but they have left to the twentieth an inheritance of labor and of duty that must not be postponed.

Dr. Salmon, veterinary of the Bureau of Animal Industry in the Department of Agriculture at Washington, makes this remark: "Without doubt there should be greater efforts than have yet been put forth in this country to secure a pure and wholesome food supply. Those who are working in this direction need the encouragement and aid of the general practitioner of medicine, I might say of the whole medical profession. They need even more than this — the experience and knowledge which are gained by the men in the active practice of medicine."

Thus does the representative of our government at Washington recognize the agency and influence of the medical profession in the prosecution of this department of its work, thereby practically making the acknowledgment that the work is not yet thoroughly nor satisfactorily done. And so, because it is not, our strong men perish, and so our children die. When sixteen children in every hundred born in Massachusetts have died before they were one year old, through twenty successive years — twenty-five in every hundred, according to high authority, is the ratio in all large American cities; — when 11,500 of the little ones die in a single year, as happened in Massachusetts, in 1900; when 47,266 persons, or very nearly one in every fifty, of the total population of the State, exclusive of those who die by violent death and also those who are reported as dying

of old age, die in Massachusetts in a single year, a large percentage of whom die from zymotic or infectious diseases, which should be largely if not wholly preventable, there is certainly something left for medical science and the twentieth century to accomplish which all the centuries that have preceded it have left incomplete. These facts appear in the registration reports for 1900.

And to him whose nature is easily moved to sympathetic sorrow for affliction, they may well be deemed sufficient to bring into activity a train of sentimental reflection that quickens the heart throbs and suffuses the eyes. The suffering and distress of those whose support is thus taken from them, and the moans of the despairing mothers of the eleven and one-half thousand babes that are dying, should burst the clouds of indifference that surround the subject asunder and cause the deaf heavens to hear.

While these unsanitary or unsafe conditions prevail, whose existence is a matter of common observation, which every man of science assures us invite disease; while these fatalities continue, as we must believe largely in consequence of such condition, faithfulness to the obligations of an honorable calling, that is charged with the great responsibility of the preservation of the public health, demands that the medical profession shall devote its energies more systematically and more determinedly to the inauguration of improved systems of sanitation that may result in better things.

And so there has been no reluctance in bringing the subject of preventive medicine before a gathering of physicians, confident in the assurance that it cannot be out of place. The times are propitious and the need is great. It must be the coming topic for discussion and for settlement and for urging upon popular support and favor, as these early years of the century begin. Its benefits are certain; its possibilities are great. Perhaps in this way we may discover the

explanation of some of the hidden mysteries of our being that seem incomprehensible now.

Perhaps we may learn why it is that the food that nourishes the body becomes its destroyer, too. Perhaps it will yet tell us why the same region that engenders the chill and the ague becomes a balm to the sensitive lung. The guiding stars of science indicate that the paths of progress lead in this direction, and undoubtedly the most encouraging and satisfactory successes of the period are to come in this way. It may win fame for no one, to be identified with the movement, in favor of sanitary science, but it will furnish an opportunity for doing a world of good. It may be less fascinating than a surgical exploit, but its beneficence may be multiplied hundreds of times. It is a marvelous achievement certainly, when it happens, and should bring renown and honor to the operator, to lift the curtain of darkness that veils the light of reason, and by a bold and hazardous surgical operation to restore a wretched and pitiable idiot to useful and rational life. It is a wonderful thing truly, and is deserving of all admiration, to suture a stab wound in the heart successfully, and so to prolong a life.

But probably these occurrences will not come very frequently into the ordinary life of the physician, and it may be that even Dr. Sternberg of New York, with all his great abilities, who was so successful in the former instance, and Dr. Nietert of the City Hospital at St. Louis, who performed the stab wound operation, may be compelled to rest their claim for exceptional distinction in these classes of cases — and they may well be content to do so — upon their success in a single case.

Opportunities for doing great things do not often happen, and perhaps in the practice of medicine, least frequently of all. But here the little things count. And he who secures to the children of Massachusetts a milk supply that shall

yield the required nutrition, and yet be free from pathogenic organisms, will save thousands of lives. He that shall devise some method of harmlessly destroying or removing decomposing material, wherein disease germs originate, will save thousands more. Whoever will find a way to prohibit the pollutions that accumulate upon the earth's surface, and that are now permitted to contaminate the water that is used for domestic purposes, from reaching their present destination, will delay the flight of uncounted numbers to the celestial regions, and will prolong their stay upon the earth. But certain questions arise and need to be answered.

When so many organizations and agencies are working so actively and so harmoniously together, with these objects in view, what reason for dissatisfaction or discontent remains? Just this one: results at present are not commensurate with humanity's needs. When peace and harmony combine so happily to work out the solution of these intricate problems that interest so many, what excuse can be given for fermenting a feeling of uneasiness and unnecessary fear? Simply this: nigh fifty thousand persons die in Massachusetts every year, at an age when the insurance tables of expectancy would have assigned to them a longer life.

But what more can be done? The one thing that more than anything else is needed is to awaken a public sentiment that, realizing and comprehending the great importance of these measures, will not only justify and encourage an expenditure that shall be adequate to the destruction of conditions that foster the development of diseases, but shall imperatively demand such expenditure and such destruction wherever disease germs are found.

If, then, efforts are properly directed, and if superintended by competent guides and directors, the closing years of the century that is opening should witness results in the

department of medicine hitherto unattainable and that shall equal or rival the surgeon's most extravagant claims. If the valleys and waste places are teeming with dangerous bacteria, they must be exalted. If the mosquito is such a dangerous creature, the place of its abiding must be destroyed. The mountains may not be brought low in every instance and the mountain wind may not be tempered by human devices nor adapted to the needs of the tubercular diathesis, but the human subject may be taught, perhaps, to protect himself from its chill. If the appearance of the milker is suspiciously or conspicuously uninviting, he must be made to see the necessity of either changing his occupation or his methods of doing his work.

And so on—and so on! Through every department of human experience there is need for constant supervision, watchfulness and care. If individual interests, or rights even, are invaded and become antagonistic to the public needs, then individual interests and rights must be made subordinate to public necessity, when public safety is the issue that is involved; and, if it be possible that whatever is for the public benefit can be detrimental to the interests of the individuals of which the public is composed, then compensation for the injury that is inflicted must be allowed.

It will be objected—and not without reason—that the expense of such a movement will be enormous; and it is true. But the loss of fifty thousand, or thereabouts, of useful and cherished Massachusetts lives per annum, is a more costly and deplorable sacrifice still. The cost is not an insuperable objection nor obstacle against the expenditure of whatsoever of treasure these improvements and reforms may require. The means can be provided to procure the laborers, and the laborers are ready to do the work, and results will fully justify—even if they do not amply repay—all the expense that need be incurred. And means can be found by which all these things can be done.

Capital can furnish means to do great things. It can tunnel mountains. It can cover a whole land with a network of railroads, even to its most inaccessible regions, and settle its waste places with a busy, thriving and prosperous people that increases its wealth and its power. It can find means to turn the waters of Niagara into a source of revenue. It can cross mighty rivers with bridges of enormous span. It can find means to flash its signals unheralded through the air. It can secure communication with most distant regions, and astonish them with messages that pass the sunlight, in their journey around the world. It can illumine its mansions with the lightning of heaven and literally hitch it to its car. It can execute and maintain enormous trusts. It can combine steamship lines. It can monopolize traffic and transportation and trade. It can spend uncounted thousands in testing and perfecting a piece of heavy ordnance, and it can build forts and arsenals without considering the cost. It can build battleships that are apparently invincible. It can create navies that are at once the wonder and admiration, as well as dread and terror, of the world. It can equip and maintain armies that no power on earth has yet been able to withstand. It can support courts and camps. It can fit out costly expeditions to most distant places to observe under most favorable conditions the occultation of eclipses or the transit of a star. It can extract luxury, as well as profit, from winter's dreary and desolate fields of ice. It can find remunerative employment in blasting out huge and shapeless masses from the barren and unimpressionable granite quarry, and transforming and fashioning them into forms of surpassing beauty, and adapting them to purposes where ornament blends with permanent use. It can dig treasure from the mines of the earth, and refine it at the smelting furnace and stamp it at the mint, in quantities that are most conveniently handled with a derrick and gathered with a spade.

It only fails of its purpose and is beaten and defeated when it attempts to find a northwest passage to reach the northern pole, and to construct a practical, safe and useful flying machine.

And the amount of money that has been lavished on either of these questionable—even if successful—projects would, it is very probable, give us all that we ask or need. And it neither satisfies or pacifies our desires nor convinces our judgment, to tell us that we transgress the bounds of prudence when we ask it to come to our assistance when so many precious lives are involved. It is idle to tell us that whatsoever of capital is demanded to secure the public health and safety—whether it be much or little—cannot be furnished without either perceptibly impairing its energies or seriously obstructing its designs.

There is no doubt, there can be none, that the ravages of disease will be greatly diminished and the death-rate correspondingly changed when suitable measures shall be adopted for carrying projects like these into execution, and thus affording protection to those who do not or cannot protect themselves. And if, indeed, their excessive cost should seem to justify a doubt of their expediency, yet the times are come upon us when every consideration and dictate of humanity, of philanthropy, of zeal for the public safety, of interest in the progress of medical science, demand that the experiment shall be no longer delayed.

It is time to end this homily, that seems but just begun. The subject is inexhaustible. But the hand will move upon the dial plate, and you are growing weary, and a more alluring entertainment is awaiting you in the hall below. The great multitude that represents the medical profession in Massachusetts are gathering, eager and impatient, to exchange those kindly greetings and interchanges of recognition that cement more strongly the sympathetic tie that binds them, as they gather round the social board. Our

annual festival does not fail. Year by year, as the sun approaches the solstice, we note — if we do not always welcome — its return. It has its delights and its enjoyments, but it has its suggestions too. Year by year we find ourselves moving farther and farther upward toward the head of that great procession that marches, not only into the anniversary dinner, but into "the pale realms of shade" as well, whither so many have preceded us whose forms and features have been familiar to us in years that are gone when we were marshaled to our places in the rear, and whom we have watched year by year, to see them gradually putting off the robes of stately manhood, to wear the crown that only age could bring them, and then to disappear, that they who joined the Society in the decade next in order, might move upward to the places that had belonged to them.

The century is at its beginning. At its close no one of us will remain. But that matters not; the world will move on. Medical science will continue its work for humanity, will make new discoveries, will find new and better ways of controlling diseases, will establish more perfect and effectual methods of arresting and destroying the causes on which they depend. Generations will come and go. The young, hopeful, ardent, eager, aspiring, ambitious graduate in medicine will take his diploma and enter upon his work with high resolve and noble purpose to win distinguished honors and achieve a brilliant success, as his predecessors for generations have hoped to do. The middle-aged practitioner will pursue his daily round of duty with a calmness and serenity of spirit that no adverse circumstance can disturb, that are born of faith in the value and usefulness of his profession and his labors, and with resignation to the many hardships of his lot, that will sustain him and reinforce his courage when the stimulus of ambition and expectancy has been withdrawn. The aged

veteran in the service will look backward reflectively over the paths that he has trodden and where his fathers have traveled before him, contrasting the new ways with his own, and then will sleep as they sleep.

But The Massachusetts Medical Society will live on; surviving all changes, keeping fully abreast with all advances, it will live on. Strong in the abiding affection and veneration of its members, stronger still in the confidence and support of the people, for whose welfare it exists, but strongest of all in the ever increasing and ever expanding range of its benefactions, it will live on and on; and if it shall be true to its history and equal to the opportunities that await it, it shall rise to higher — and still higher — planes of usefulness and honor, and the voice of millions shall reverberate down the ages in strains of thanksgiving, gratitude and praises for all the blessings it has secured to men.

ARTICLE II.

THE SHATTUCK LECTURE.

THE CHANGES IN THE SPINAL CORD AND MEDULLA IN PERNICIOUS ANÆMIA.

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DELIVERED JUNE 10, 1902.

THE CHANGES IN THE SPINAL CORD AND MEDULLA IN PERNICIOUS ANÆMIA.

I DESIRE to express my appreciation of the honor conferred upon me by the officers of The Massachusetts Medical Society in inviting me to deliver the Shattuck Lecture. Many eminent men have preceded me in this honorable position, and it was with profound trepidation that I accepted the responsibility which precedent had established in giving a high tone to this lectureship. At the time I accepted the invitation to deliver the lecture, I was deeply interested in the subject which I chose for my paper. Pernicious anæmia has been the subject of considerable research work in a clinical way for several years, and as I have had the opportunity of observing the nervous phenomena and lesions of the nervous apparatus which may occur in this disease, I felt that it would be a proper subject for further investigation and report upon this occasion.

Lichtheim was given the credit for recognizing, for the first time, the spinal cord changes which may occur in pernicious anæmia. His report was based upon two cases, and was confined entirely to the changes of the cord in pernicious anæmia. Soon after this his student, Walter Minnich, reported one case in an inaugural dissertation.

The next important contribution was made by James J. Putnam, of Boston, who, in 1891, reported "A Group of Cases of Sclerosis of the Spinal Cord, Associated with Diffuse Collateral Degeneration Occurring in Enfeebled

Persons Past Middle Life, and Especially in Women; Studied with Particular Reference to Etiology." In this paper Putnam mentions Lichtheim's observations of cord changes in pernicious anæmia, and notes the similarity of the cord changes as compared with those observed in his patients. He also mentions the fact that Lichtheim ascribed the cord changes to some blood-circulating toxin. Putnam apparently accepted the toxic theory as the chief etiologic factor in his cases, but he emphasized the fact that the age, the sex, the neuropathic inheritance, poor physical development, nutritional disorders, diarrhœa, etc., were important contributory causes. Putnam's paper did not receive the attention which it merited in Europe. The title was perhaps misleading, in that no mention was made of pernicious anæmia.

Since 1891 many contributions upon the subject of the spinal cord changes of pernicious anæmia have been made abroad and in this country. Nonne; Minnich; Bastianelli; Burr; Russell, Batten, and Collier; Lloyd; Taylor; von Voss; vonNoorden, and many others have reported cases and have described the clinical phenomena, the gross and histologic changes which have been found in conditions usually associated with pernicious anæmia, and also in conditions in which the blood condition was not like that of pernicious anæmia. Similar cord changes have also been noted and reported by some of the above-named authors, and by others in leukæmia, lathyrism, pellagra, diabetes mellitus, diphtheria, and associated with the degenerations incident to poisoning by phosphorus, lead and arsenic.

In 1902, Putnam made, with the collaboration of E. W. Taylor, a second report upon "Diffuse Degeneration of the Spinal Cord," in which a review and comparison of the views of many writers were noted. In this article Dr. Taylor made a most admirable report upon the pathologic anatomy of the nervous apparatus.

In view of these facts, I approach the subject with hesitancy, and proceed only with the hope that an additional report may be interesting, if it does not add anything new to the subject. My observations have been limited to the diffuse degenerations of the spinal cord of pernicious anæmia. The similarity of the cord lesions of pernicious anæmia and of the conditions enumerated by Putnam, by Russell, Batten and Collier and also by other reporters, is so striking that it must impress anyone who reviews the extensive literature of the subject. The similarity of the contributory etiologic factors of pernicious anæmia and of other conditions in which diffuse degeneration of the spinal cord occurs, as reported by Putnam especially, is notable. It is my object to present cases of pernicious anæmia, with the now recognized characteristic changes in the spinal cord, and to discuss somewhat the similarity of the cord changes in pernicious anæmia and in the other conditions above-mentioned.

It is possible that some of the earlier reported cases of diffuse degeneration of the spinal cord were not ascribed to an existing, but unrecognized, pernicious anæmia. It is just as likely, however, that some of the cases reported as pernicious anæmia, with cord changes, were not typical cases of that disease. This statement is based upon the fact that in some of the earlier cases reported the blood state is not noted sufficiently to enable one to recognize the case as one of a pernicious type of anæmia; and, furthermore, at the time when the first reports were made, the means of recognition and differentiation of the anæmias were not as clear as now. In earlier times pernicious anæmia was diagnosed upon the basis of a profound progressive anæmia, associated with a tendency to subcutaneous, submucous and retinal hemorrhages. When to these symptoms was added the post mortem finding of a characteristic, pale, anæmic fatty state of the organs, and especially of the heart, with an increase of iron in the liver, as was noted by

Quincke, and the presence of degenerated bone marrow, the diagnosis was certain. We now have a better knowledge of the morphology of the blood. We are able to separate the primary from the secondary anæmias, and the blood of the pernicious type is characteristic and easily recognized. When we find a marked oligocythæmia, with the presence of many poikilocytes, microcytes, and macrocytes, with polychromatophilia, and a relatively high percentage of hæmoglobin, with a normal or a higher than normal color index, it affords a type of blood which is found in no other anæmic state. If to this we add the presence of nucleated red cells, and especially of megaloblasts, the fetal type of red cell and of a few myelocytes which together indicate degeneration of bone marrow, the diagnosis is reasonably certain without a postmortem examination.

Cause.—Our knowledge of the cause of pernicious anæmia is not satisfactory. We recognize certain contributory factors and infer a condition of toxæmia which has not been proved. We know that the disease may occur in certain conditions; that these conditions are so frequently present in individuals who do not develop the disease, that we must look upon them as accidental.

Pregnancy, associated with constant vomiting and malnutrition, has been followed by pernicious anæmia. After repeated or severe hemorrhages, and in individuals who are poorly nourished, especially when the poor nutritional condition is associated with some error of physical development, a grave anæmia may follow. It is associated with, or follows most frequently, disease of the digestive apparatus. It has been known to follow syphilis and malaria, and in a few instances typhoid fever and la grippe seemed to be associated causes. In one of my cases the patient suffered from scarlet fever, was not well thereafter, and two years later developed the typical disease. The disease so infrequently follows the infections, like syphilis, malaria,

typhoid, la grippe, etc., that it is impossible to ascribe any specific relation to it.

In Berlin in ten years there were admitted to the hospital 148,000 patients, of whom 274 were diagnosed as pernicious anæmia, that is about .2 per cent. of all of the hospital cases. It is also stated that 22.4 per cent. of the above cases showed evidences of syphilis. This indicates that a small percentage of syphilitic patients develop pernicious anæmia. It is said, that individuals of a weak constitution and poor development are especially liable to the disease, yet the strong and full-blooded may develop it. The disease occurs more frequently in women than in men. Of the 274 cases mentioned, 172 were women, and 102 men. It may occur at any age, but it is more common in the fourth decade than at any other time in life. It has long been observed that the disease is frequently associated with some disturbance of the digestive tract. This consists in many instances of an atrophic condition of the walls of the stomach and of the intestine, which may become much thinned. The glandular elements of the mucous membrane have been found much atrophied. Achylia gastrica exists in many instances. In carcinoma of the stomach and in chronic glandular gastritis the same atrophy of the stomach wall may occur without pernicious anæmia. An atrophic condition is also found in other portions of the body, and it is not at all unlikely that the condition of the stomach and the intestinal wall has only a contributory etiologic relation to the disease. Intestinal parasites have been found present in the disease, especially the bothriocephalus latus, and the ankylostoma duodenale. The disease, however, does not develop in all cases in which these parasites are present. That they have some relation to the disease is shown by the fact that some patients, who present characteristic blood findings, get well when the parasite is discharged by the patient. The condition of profound

anæmia, with deformed red cells, the presence of fetal red cells in the circulating blood, the presence of an abnormal amount of iron in the liver, together with degenerative changes in the muscles and in the marrow of bone, is a reasonable hypothesis for the assumption of the presence of a circulating poison—a hæmolytic toxin as the fundamental cause of the disease. The source of the poison has been the subject of many theories.

Schauman and Tallquist have brought forward the theory that in cases of pernicious anæmia due to the bothriocephalus latus, the parasite was the source of a toxin because the worm itself was either sick or dead in the host. In this condition the worm was the source of the toxin which produced the anæmia, and explained to these reporters the reason for the non-development of pernicious anæmia in most cases of individuals affected with the bothriocephalus latus. They made the following experiment: The extract or bruised links of the bothriocephalus latus was fed to dogs. In one dog, after two weeks, marked anæmia occurred, the red cells falling from 7,200,000 to 3,400,000.

The view has been advanced that the toxin is of bacterial origin, but no definite observations have been made which would establish this, with the exception of the research work done by Professor Adami and his pupils. In his studies upon hypertrophic cirrhosis of the liver and on hæmochromatosis in pernicious anæmia, Adami advances the opinion that many of the sclerotic processes of the body are due to acute and chronic infection through the presence in the tissues of bacteria or bacterial poisons, which may be derived from the intestines. The organism is successful in overcoming the general infection, but the tissue cells become exhausted through the specific infection, with resulting degeneration and connective tissue hyperplasia. He described diplococcoid forms of bacteria in the stomach and intestinal walls, and in the liver cells, which he believed to

be altered colon bacilli. The suggestion of Adami is very pertinent, especially if one believes that pernicious anæmia is due to a toxin.

More recently Hunter, who had confirmed the observations of Quincke and others as to the hæmolytic effect of a possible toxin, suggested the origin of the poison in the intestinal canal from the altered anatomic condition of the stomach and bowel. Still later, Hunter has suggested the idea that the toxin and the condition of the gastro-intestinal canal have their source in a foul condition of the mouth of the patient, who suffers from pernicious anæmia, and he notes the condition of carious teeth, gingivitis, etc., in many of his patients. It is not improbable that from the evidence we have, pernicious anæmia is due to some hæmolytic toxin, but whether of bacterial or autogenetic origin we cannot at present definitely say.

Symptoms. — The symptoms of pernicious anæmia are chiefly those due to a profound anæmia manifested by weakness, lessened endurance, with dizziness, dyspnœa, palpitation of the heart, etc. The disease is not necessarily steadily progressive, as it is interrupted by waves of improvement. It may be either very acute or chronic in its course, as cases are known to die within a few weeks and others to live for several years. In probably a large percentage of the cases the body weight is preserved, and in a few others there is a loss of weight, and in some emaciation. There is a tendency to subcutaneous, submucous, retinal and other hemorrhages. Gastro-intestinal disturbance is very commonly present, and especially diarrhœa. The appetite is often poor or capricious. There may be a craving for acids, salts, or some other thing with decisive taste. The disease is associated with nervous phenomena in the great majority of cases; usually, however, subjective in character. There is often paræsthesia of the feet, sometimes of the hands. Cerebral disturbance in the form of lessened cerebration

may occur, and Minnich has observed fixation of the pupils, while Bemis, Müller and Nonne report cases of slight facial paralysis, or of one-sided paræsthesia, aphasia, etc.

The spinal cord lesions, which are now recognized as occurring in a small percentage of the cases, may appear as one of the earliest manifestations of the disease, or they may occur late, and may be manifested slightly or not at all up to the time of death. The spinal cord lesions, which will be discussed more fully later, do not always bear a direct ratio to the symptoms. In some instances, with no perceptible spinal cord changes, there may be marked subjective paræsthesias, and even slight ataxia and spasticity, while in other cases, with marked cord changes, the symptoms may be subjective only, and not in keeping with the marked changes in the cord. In the severer degrees of spinal cord lesions, when established, the course of the disease is usually rapid to a fatal ending.

Bastianelli divides the cases into two groups. One in which the pernicious anæmia is the predominant factor, and the cord lesion developing late, with only slight clinical manifestations, or possibly with only subjective disturbances. The second, in which the nervous phenomena develop first or coincidently with the anæmia, the nervous phenomena predominating and overshadowing the symptoms due to the anæmia, and the case rapidly developing into one of paraplegia, with flaccid muscles, loss of sphincter control, bed-sores, and death within a few weeks.

This makes a fairly good division for clinical use, but not all cases can be placed within the two divisions.

During the last few years I have had under direct observation thirty-six cases of pernicious anæmia.

Analysis of my Group of Thirty-six Cases. — Of these, 20 were males, and 16 females. The preponderance of males over females is unusual. The ages were as follows: There were three in the second decennium, seven

in the third, ten in the fourth, nine in the fifth, and seven in the sixth. The youngest was 24 years of age, and the oldest 69. In four cases tuberculosis existed in the family within two generations. In two patients carcinoma existed in the family within two generations. Three of the patients had suffered from syphilis. In two a severe attack of grippe was apparently the cause of an illness which finally developed into pernicious anæmia. One was overcome by smoke and gas at a fire in a large building. One suffered from exophthalmic goitre; fourteen had suffered from diarrhœa as the first manifestation of the disease. Diarrhœa had existed for three years as the longest time, and in one, after an acute attack of diarrhœa, the disease suddenly developed. Constipation, obstinate in character, occurred in two. Nausea and vomiting were present in the majority of the patients at some time in the course of the early stage of the disease. Loss of appetite, amounting in some to complete anorexia, was present in all, and a capricious appetite existed in a few. In twelve repeated examinations of the stomach contents were made after test meals, and disclosed the absence of hydrochloric acid in all. There was enteroptosis of a severe degree in three. The association of grippe with pernicious anæmia has been noted by Bastianelli and by Putnam as a contributory factor in spinal cord lesions. In all patients two or more complete blood examinations were made which gave the characteristic blood condition of pernicious anæmia. Retinal hemorrhages occurred in four; subcutaneous and submucous hemorrhages occurred in eight; epistaxis was a not uncommon factor, and in several bleeding of the gums was observed. Of the twenty male patients, in only three were the teeth bad, and in four only was the mouth in an unhealthy condition. Of the women, the condition of the mouth and teeth was notably bad. In two cases no note was made of the condition of the teeth and mouth; in the remaining fourteen women, either the

teeth were decayed or the gums were infected in all. Of the thirty-six cases paræsthesia occurred early in the history of the case. It appeared simultaneously in the feet and hands in all of these cases, with the exception that in one it was confined to the toes of one foot and to the ulnar border of one hand. In four female patients the anæmia preceded the nervous phenomena by two years and four months, and in two men the nervous phenomena appeared one year and six months, respectively, after evidences of pernicious anæmia were established. In twenty-four of the cases the nervous phenomena were confined to subjective disturbance only, manifested by tingling, numbness, coldness, heat, formication, etc., of the hands or feet, or of both, and in one instance involving the entire lower extremities. Hyperæsthesia occurred in one. Disturbed temperature sense, inability to recognize quickly heat and cold, occurred in two, otherwise objectively the sensation of the feet and hands was normal. In these twenty-four cases the patients complained frequently of discomfort, of a sense of numbness and tingling which were most often present, and usually this condition remained, even when the patients were on a wave of improvement. In only one did it disappear during a wave of improvement, and it reappeared when a relapse occurred. In ten patients the subjective nervous disturbance was associated with a spastic, and usually with an ataxic condition, which grew steadily worse, and in three developed into complete flaccid paraplegia, with loss of knee jerk, loss of voluntary bowel and bladder control, and the development of œdema and bed-sores. One died while in a state of spastic paraplegia. Four are living with spastic ataxic paraplegia, and very troublesome subjective sensory disturbances. One case died with simple sensory disturbances only without ataxia, spasticity, or paraplegia, whose spinal cord showed dorso-lateral sclerosis. One died with all the evidences of insanity, but a post mortem was not obtainable.

Burr states that a girdle sensation is never present in the spinal cord lesions of pernicious anæmia. Girdle sensation was present in all of the cases which developed spasticity in my group. As observed by Putnam, some of the patients complained of a peculiar paræsthesia of the mouth. One complained of a sensation as if his mouth contained threads of hard cotton; another that his mouth had the sensation of dryness, although saliva was present; another of a sense of burning which cold water did not allay. The sexual power of the males was uniformly diminished. Menstruation ceased, as a rule, in all of the younger women. Severe spinal cord lesions occurred in the female cases, in the relatively young. One was thirty-nine, one forty, one forty-seven, and one thirty-four years of age. In the males severe cord symptoms occurred in one individual of thirty-six, another of forty, in two of fifty-six, in one of twenty-six, and in one of sixty-two.

Case 1. — Female; aged 60; a copyist; the mother of two children; was admitted to Cook County Hospital in my service on September 8, 1899. The patient stated that for six months she had had great weakness, with faintness, shortness of breath, palpitation of the heart, cold extremities, and general nervousness. She had twice had severe nosebleed. There had been no loss of weight, but she had become so weak that she was finally obliged to remain in bed. She complained of numbness of the hands and feet, with a sense of burning.

Examination showed a lemon-yellow pallor, with pale mucous membranes, and some œdema of the eyelids and of the ankles. The radial pulse was small, rapid, compressible and weak. There was a soft systolic murmur at the base of the heart, which was transmitted into the vessels of the neck. A venous hum was heard in the neck. The spleen was palpable, and the liver extended two fingers' breadth below the costal arch. The pelvis was negative.

The urine was normal. The temperature was slightly elevated, and the patient stated that it had been so for two days following an attack of diarrhœa. The stools were watery and loose, but contained no parasites. The blood showed 960,000 reds; 25 per cent. hæmoglobin; 3,000 white cells; 36 mucleated cells were estimated per cubic millimeter, of which 12 were megaloblasts, 12 microblasts, and 12 normoblasts. The knee jerks were normal. There was no ankle clonus, and no objective sensory disturbance of the skin. The eye grounds were normal. The patient, with rest and simple food and restorative tonics, gradually improved and left the hospital after one month. She again came under my care at the Presbyterian Hospital on the 26th of March, 1901, and remained under observation constantly until the time of her death, on the 24th of July, 1901. Frequent examinations of the blood were made, which showed the typic condition of pernicious anæmia, with waves of improvement and decline until the death of the patient. The stomach contents, examined on several occasions, showed always absence of hydrochloric acid. No bacteria were ever cultivated from the stomach contents. The stools showed no parasites at any time. The patient complained a great deal of a sense of numbness and tingling and burning in the feet, and of numbness and tingling in the fingers, but beyond this there was never any evidence of a disease of the nervous apparatus. She died after two days of unconsciousness, without fever, and without the occurrence of any acute infection.

A post mortem was made by Professor LeCount, of Rush Medical College, nine hours after death. There was atheroma of the coronary arteries and of the aorta; parenchymatous degeneration of the myocardium, which was very pale; hydropericardium; œdema of the lungs; hyperplasia of the spleen and liver; fatty degeneration of the liver; follicular enteritis and colitis; ascites; general anæ-

mia; icteroid pigmentation of the whole body, and sclerosis of the posterior and lateral columns of the spinal cord, which were examined histologically. A complete histologic examination was made of all of the organs of the body, which showed degenerative changes in the blood vessels, in the heart muscle, the liver, in which there was also an increase of the iron by Quincke's test, of the spleen, atrophy of the stomach and intestinal walls, with follicular degeneration of the mucous membrane of the intestine, and lymphoid degeneration of the marrow. The marrow contained red cells, normoblasts, and megaloblasts, and irregular and spindle-shaped cells.

Histologic changes of other organs are unimportant, and are omitted for the sake of brevity.

Sections of the spinal cord were stained by Marchi, Van Gieson, toludin blue, and Weigert-Pal methods. Slight sclerosis was observed in the lateral tracts in the lower lumbar, which increased gradually as one passed upward into the upper cervical region. Slight sclerosis was noted in the posterior median columns in the mid-thoracic region, which was increased to a maximum in the cervical swelling. Above this point the sclerosis gradually disappeared, and at the junction of the cord and the medulla had entirely disappeared, as is shown in the accompanying figures. In the thoracic cord the Nissl bodies of the anterior horn cells were normal. The blood vessels of the cord contained many lymphocytes, and many others leucocytes. The walls of the blood vessels were unchanged. In Clarke's nucleus the nuclei were centrally placed, and the Nissl bodies stained normally. In the cervical swelling there appeared to be a slight breaking up of the Nissl bodies in the anterior horn cells; otherwise the condition was the same as that in the thoracic cord. Spinal roots, ganglia, pia and vessels normal. (See cuts of Case 1.).

The cerebral cortex showed normal cells and Nissl bodies.

This case is one which belongs to the group described by Lichtheim, Nonne, and Minnich, and would belong to the first group as classified by Bastianelli. Contrary to the rule, the lateral tracts were more extensively involved than the posterior, although the process extended to a severer degree in the posterior than in the lateral tracts. The microscope showed very definitely in this case the cribriform result of a falling out of the degenerated nerve fibres in the lateral columns without a glia increase. In the cervical swelling the neuroglia increase is evident with resulting sclerosis.

Case 2.—Female; aged 39; American; has borne four children; was admitted to the Presbyterian Hospital on July 26, 1900. Her mother died of paralysis, the nature of which the patient does not know. Her father is still living, but has suffered from stomach trouble a great deal of his life. One maternal aunt died of cancer. One brother and seven sisters are living and in good health. The patient has worked hard, and has always been in good health until the birth of her last child. Two of her children are living, and two are dead. One was still-born. Her present illness began seven years ago, after the birth of her last child. She suffered from indigestion with gaseous eructations, acidity, with fullness in the epigastrium after eating. Occasionally there was vomiting, which had no relation to the meals. She never vomited blood. The appetite was variable; sometimes it was good. She had diarrhœa, with watery stools, which often contained undigested food, for the first six weeks of her illness. She was weak, and a noticeable pallor occurred a year ago. She does not remember when her first nervous symptoms occurred, but states that recently she had had a sense of numbness about the hands and feet, with irritation of the bladder which required her to urinate frequently. There was also a sense of pressure in the rectum, and sometimes

pain, especially if the bowels were constipated. Menstruation ceased four months ago. Her strength and endurance have been gradually lost. She noticed the legs were becoming weak during the fall of 1899, and after December, 1899, she was obliged to remain in bed. Her legs became gradually weaker, and she finally lost complete control of the bladder. Her bowels were usually constipated, and required a laxative or the injection of water to move them. The pallor, she states, increased, and there was a decidedly yellow tint to the skin.

Examination revealed a woman fairly well nourished; skin icteroid and pale; mucous membrane very pale; the tongue indented and furred; no carious teeth, and the gums healthy. The chest negative, excepting for a soft systolic blowing murmur heard in the mitral and pulmonic areas. Pulse, 100 per minute, small and soft; the belly of pot form, with diastasis of the recti muscles. There was very palpable visceroptosis, including the liver, kidneys, spleen, intestines, and stomach. The uterus was retroverted and moderately fixed. Rectum was normal; the eye grounds were normal. There were slight bedsores over the sacrum; the patient lay in bed with the thighs and legs flexed and closely approximated. There was some atrophy of the muscles of the lower extremities, chiefly of the extensor groups. The knee jerk was increased; there was slight ankle clonus, more of the left than of the right; Babinski's sign was present in the right, but not in the left. There was slight anæsthesia of the feet and legs; muscular sense was absent; there was some disturbance of the temperature sense in certain areas, where both heat and cold were felt as touch only. There was a small area in the left inguinal region and upon the outer surface of the right thigh where heat was felt as pain. There was a noticeable loss of power in the legs, and this was more marked in the right than in the left. The sphincters of the rectum and bladder were

relaxed. The muscular power and sensation of the skin in the upper extremities seemed normal. The patient complained of a sense of constriction about the abdomen in the waist line. The eye grounds were normal. The stomach contents contained no hydrochloric acid. The urine was normal. The blood showed 610,000 red cells; 20 per cent. of hæmoglobin, and 3,200 whites. Poikilocytosis was very marked, and there were many nucleated cells, the megaloblasts predominating. The patient remained in the hospital from that time until the date of death, February 27, 1901. Repeated blood examinations confirmed the diagnosis of pernicious anæmia. The spinal cord involvement increased; and finally resulted in flaccid paraplegia. The patient's mental condition remained fairly clear, although she did not apparently recognize the gravity of her condition. The arms became finally ataxic, although the patient was, at all times, able to feed herself and to button her clothes; but there was a distinct loss of power in the upper extremities. The sensations objectively remained normal, which was a most curious phenomenon when one notes the condition of the cervical spinal cord. There was at no time any disturbance of the eye grounds, nor of the oculomotor muscles. The appetite remained fairly good; the patient vomited occasionally. The stomach contents showed no hydrochloric acid upon repeated examinations. During the latter part of the patient's illness she developed a cystitis, and later a pyonephrosis. As a result, there was an irregular type of fever, with a rapid pulse, until death closed the scene.

The post mortem examination was made by Professor Hektoen, of Rush Medical College, at the Presbyterian Hospital two hours after death. There was general anæmia; red marrow in the femur; splenic hyperplasia; ascending cysto-uretero-pyelonephritis; broncho-pneumonia; fatty liver; sclerosis of the aorta; chronic pelvic peritoni-

tis, with tubo-ovarian adhesions; extensive sacral decubitus, œdema of the feet, and degeneration of the postero-lateral columns of the spinal cord. The microscope revealed the usual extensive atrophic and fatty degeneration of the organs of the body, and the liver an increase of iron by the Quincke test. For the sake of brevity, I will omit a further histologic description of the organs, excepting of the spinal cord. Sections of the spinal cord were stained with toludin blue, VanGieson and Marchi. The Weigert-Pal method showed degenerative changes, with absence of the myeline sheath from many of the tracts. At about the fourth lumbar, the posterior column was invaded, leaving, however, normal white tissue in proximity to the gray matter and presenting undegenerated the oval bundle of Flechsig. The lateral tracts showed degeneration in the right more than in the left. In the lower dorsal region the degeneration had become more extensive, involving almost completely the posterior columns, but leaving a narrow normal band of white tissue next to the gray commissure and posterior horn; the lateral tracts were almost equally involved, and one direct pyramidal tract, showing extensive degeneration. The posterior root zone and the column of Lissauer remained normal. In the mid-dorsal region the sclerosis was very extensive, involving, to a greater or less extent, the complete periphery of the cord. The sclerosis still left a slight band of tissue next to the gray commissure, and a larger band along the posterior gray horns. The columns of Goll and Burdach were almost completely involved, while in the lateral column the pyramidal tract, the direct cerebellar tract and Gowers' tract were sclerosed. Both direct pyramidal tracts were implicated. In the cervical swelling the posterior column, and in the lateral columns the pyramidal tracts, the direct cerebellar tract, and in the anterior column the direct pyramidal tracts, were extensively sclerosed. Here, again, the root zone and the

column of Lissauer were practically free. At the junction of the cervical cord and the medulla one pyramid was extensively involved, while the other one but slightly degenerated. Both direct cerebellar tracts were degenerated. The column of Goll was entirely degenerated, except at the gray commissure. The medial and posterior parts of the column of Burdach were completely degenerated, while the lateral portion was unchanged. The spinal tract of the trigeminus was not affected. In the lower medulla one pyramid was much degenerated, the other slightly so. The upper extremity of Goll was completely degenerated. The column of Burdach, excepting a small triangular area, between the nucleus of Burdach and the posterior horn, was degenerated. The internal arcuate fibers, which go to the lemniscus, were normal. In the medulla the pyramids were practically normal, there being only a slight, if any, degeneration. The white matter around the spinal extremity of the olive was normal. The internal arcuate fibres and the interolivary layer of the lemniscus were normal; likewise the external arcuate fibres. In the upper medulla the white fibres of the hypoglossal nerve were normal. Both pyramids were slightly and diffusely degenerated, more upon one side than upon the other. The white cap of the olivary body was normal. The olivary fibres to the restiform bodies were normal. The restiform bodies were normal. The tractus solitarius was normal. A section through the lower pons showed a marked degeneration of both pyramids, one slightly more than the other. This is remarkable, because of the absence of degeneration in the pyramids of sections below. The posterior longitudinal bundle was normal. The middle cerebellar peduncle was normal. The upper pons in the dorsal region showed the normal stain; the posterior longitudinal bundle was well marked. The central tegmentary path appeared normal. The medial and lateral lemniscus were normal, as was also

a section of the superior cerebellar peduncle. In the ventral pons long fasciculi, including the fibers of the pyramidal tract, and those from the cerebrum to the pons, appeared normal. The transverse fibers of the pons stained intensely. This section showed a light line in its most ventral part, which may be an artefact. In the thoracic cord the anterior horn cells and the Nissl bodies in the centre were round and granular, while in the periphery of the cell the Nissl bodies were spindle-shaped and stained normally. In Clarke's nucleus cells were small and much less in number. The cells of the lateral horn showed disintegration of the Nissl bodies. The blood-vessels contained leucocytes, but in smaller number than in Case 1. In the cervical region the Nissl bodies were in the same condition in the anterior horn as in the thoracic cord; they were spherical and fairly granular instead of spindle-shaped. Lipochrome masses filled up the cells and pushed the Nissl bodies towards the periphery. The blood-vessels were only slightly thickened. Section of the cortex showed that the pyramidal cells stain and have normal stainable Nissl bodies. (See cuts of Case 2.)

The cells of the cerebral cortex were normal.

The accompanying microphotographs of sections of the entire cord, medulla, and part of the pons, speak more definitely than words of the extensive degenerative process which occurred. It is remarkable that the nervous symptoms, especially of the upper half of the body, were so mild, as compared with the lesions of the upper cord and medulla. The absence of any marked degeneration of the gray matter, of the root zone, of the root ganglia, and of the brain cortex, agrees with the reports of others and illustrates the fact that a whole neuron, either afferent or efferent, is not involved. It is further remarkable that the lesion varies so in its extent, both lateral and vertical, in the cord; that the degeneration, especially in the anterior pyra-

mids, appears and disappears at different heights; that the degeneration is greatest in the upper and lower cervical regions, as in many of the cases reported by others. Still here, as elsewhere, in the cord, it is not confined entirely to any one of the different cord systems. A chronic degenerative process, manifested by nerve degeneration and glia increase, occurs in some part of the cord, while in other parts there is a rapid process of nerve fibre degeneration and destruction without much glia increase, leaving a cribriform-like appearance.

Case 3. — Was admitted to the Presbyterian Hospital, Chicago, April 12, 1902, in the service of Prof. James B. Herrick, who very kindly permitted me to report it. Female; aged 47; married; mother of one child, has had one miscarriage, which she says was caused by overwork. Her mother died at 62 of Bright's disease. Her father is living, at the age of 74. Ten of the eleven children of her parents' family are living, and all in good health except herself. One brother died as the result of a severe injury; one uncle died of tuberculosis, and two uncles died of kidney disease. The patient suffered from the usual diseases of childhood. Her menses were established at thirteen, and were always regular. She was married at the age of thirty. Her health has always been good. She had some stomach trouble six years ago for about three months, and from which she recovered and remained well until a year ago, at which time she observed a numb feeling in the fingertips, which increased and has remained until now. Three months later there appeared a sense of burning in the lower extremities, and the feet and ankles swelled slightly. During the last summer she noticed she was losing strength and had a poor appetite. She became pale, although she states that she has never had a good color. Six months ago her skin became yellowish. For a year she has had a sense of fullness in the stomach after meals, but never pain.

No nausea or vomiting. The bowels have been constipated. She has grown steadily weaker, and especially during the last six or seven weeks has been unable to go about. During the last year the patient says she has lost forty-five pounds. She complains at this time of numbness and tingling in the hands and forearms and her fingers feel like sticks of wood when she strikes them against objects. She has lost all power in the lower extremities, with a sense of pressure and tenderness in both groins. The legs are flexed on the thighs, the thighs upon the abdomen, and the knees are held closely together. There is some cramp-like pain in the thighs, periodic in character. There is a sense of numbness in the lower extremities. She cannot tell the position of the limbs without looking at them.

EXAMINATION. — The patient is fairly well nourished. There seems to be but little loss of subcutaneous fat; the skin is yellowish-white; the skin of the lower extremities appears waxy. There is a considerable œdema of the feet and ankles. The mucous membranes are very pale; the teeth are in bad condition; the gums are sore. The chest is negative, excepting for a hæmic murmur over the base of the heart. There is considerable tenderness on pressure in the middle of the sternum. The abdomen is tender upon pressure everywhere. The lower extremities show considerable atrophy of the muscles; the flexors are soft and flabby, while the adductors and extensors are contracted. The knee-jerks are increased: ankle clonus is present; Babinski's sign is absent. The sense of touch is everywhere acute. Temperature sense of the feet and legs is impaired; pain sense is acute over the skin of the lower extremities. The muscle sense is impaired in both the upper and lower extremities. There is apparently no loss of strength in the upper extremities, but the patient cannot tell the nature of objects placed in her hands, and they are often dropped to the floor. Patient is unable to stand or walk. The pupil-

lary reflexes are normal; the eye grounds are unchanged. Stomach contents shows absence of hydrochloric acid. The urine is normal, excepting for a trace of albumin. The patient was very weak, with a temperature ranging from 99½° to 101°, and pulse from 100 to 112. There was no appetite. Patient was able to digest without great discomfort the food that was given her.

On April 25, a blood examination showed 1,248,000 reds; 30 per cent. hæmoglobin, and 4,400 whites; many nucleated cells were found, including a preponderance of megaloblasts. Practically the same blood condition was found on May 7. The final blood examination, on May 17, showed 912,000 reds; 29 per cent. hæmoglobin, and 4,200 whites. The patient's general condition became gradually worse; the lower extremities, while practically paralized, were never entirely flaccid. The patient did not lose control of the bowels or of the bladder, although it was necessary to catheterize the bladder at times. She suffered from considerable pain in the lower extremities, and there was a gradual loss of power in the arms and hands. She died on the 20th of May. An autopsy was held one-half hour after death by Dr. Bassoe, the resident pathologist of the hospital. There was general anæmia, with œdema and atalectasis of lower lobes of both lungs; fatty liver and heart; chronic interstitial nephritis; atrophy of the mucosa of the small and large intestines; atheroma of the aorta; ecchymoses of the dura mater; œdema of the lung; and degeneration of the posterior and lateral columns of the spinal cord.

Sections of the spinal cord stained with Weigert-Pal, Marchi, vanGieson, and toluidin blue showed extensive sclerosis of the posterior columns of characteristic outline. In the upper lumbar the process involved the middle portion of the posterior column and left normal white tissue along the gray commissure and posterior horn, and in the

lateral columns involved the crossed pyramidal tracts. The column of Lissauer and the root zone were, as usual, free. The spinal gray, including the column of Clarke, was free. At the level of the second thoracic the process was very extensive in the posterior columns, but left a narrow band of apparently normal white tissue next to the gray commissure and along the posterior horn. The sclerotic process was dense about midway between the commissure and the periphery, and on each side next to the septum. The sclerotic process involved also the greater portion of the lateral tract, extending forward as far as the gray commissure. There was apparently slight degeneration, also, in the anterior pyramids, more upon one than the other side. At the level of the fourth cervical the sclerotic process was about the same as at the second thoracic, but was more extensive in the posterior columns, with a greater amount of sclerosis. A narrow band of white normal tissue was still left along the posterior horns and the gray commissure. The column of Lissauer and the root zone were free. The direct pyramidal tracts were apparently slightly implicated. The process in the lateral tract was not quite as extensive as at the lower level, but involved the crossed pyramidal, the direct cerebellar, Gowers' tract, and extended diffusely into other portions of the lateral tract. There was no thickening of the blood-vessels; no hemorrhages were found, and there were no evidences of meningitis. The ganglia and the posterior roots were normal. The degenerative process in this cord does not differ histologically from that of Case 2, or from that described by Taylor and others. The two processes so often mentioned are typically seen: The one a cribriform-like arrangement, as if a degenerated nerve and sheath had dropped out, leaving a sieve-like neuroglia meshwork, while in other portions an evident glia increase is noted, with a sclerosis corresponding to it. (See cuts of Case 3.)

The cells of the cerebral cortex were normal.

General Remarks upon the Cord Lesions of Pernicious Anæmia. — These three reported cases illustrate very typically the sclerosis of the spinal cord which has been described by so many writers as characteristic of pernicious anæmia. This consists of a sclerosis of the posterior columns, either alone or associated with a like change in the lateral tracts of the cord. The lesions in the posterior and lateral tracts are most marked, as a rule, in the upper thoracic and lower cervical regions. From this point the degeneration usually diminishes, as one passes downward in the cord, to disappear in the lower lumbar region. It also diminishes as one passes upward into the medulla. The sclerosis is not confined, however, to these two systems. In the lateral tract it may not involve the whole of the crossed pyramidal, and may invade at the same time other portions of the lateral tract wholly or partly. The crossed pyramidal tract may be free. It is not always diffuse, but may occur in focal areas, not necessarily symmetrically distributed. The anterior pyramids may be involved at different heights, not always symmetrically. Case 2 is a good example of the focal distribution, as the degenerative areas are seen to appear and disappear at different heights in the anterior pyramids. The case is also a good example of the diffuse type of the disease, inasmuch as in certain areas, at certain heights, it is very diffuse, and in others it involves the whole or part of a tract. The process is thus characterized by the fact that it does not involve apparently a whole system centrifugally or centripetally, at all levels of the spinal cord. It is apt to diffuse itself from one tract into another, especially where it is most extensive, as in the upper thoracic region. The spinal gray is usually not involved, although in a few cases reported a note is made of slight hemorrhages into the gray matter and of thickening of the walls of the blood vessels.

At most, there has been found some over-pigmentation of the Nissl bodies, and a diminution in the cells in the column of Clarke.

Another characteristic of the sclerosis is the fact that the whole neuron is not involved. In the reports made by most writers the posterior ganglia have been found unaltered, and the posterior root zone is practically always free. The cells in the anterior gray horns are practically unchanged. Mott reports two cases in which he states he found degeneration of the cells of the cortex, associated with descending degeneration downward through the pyramids, but the great majority of reporters have found the cells of the cortex practically normal. The process is usually not attended with degeneration of the peripheral nerves, although a few individual cases have shown slight changes in the axis cylinders. The process is, therefore, one in which certain systems are partly involved, with a tendency for the degenerative process to diffuse itself through other tracts at certain levels. It cannot be called systemic, because an entire system is not involved, nor is a whole neuron implicated in the degenerative process.

E. W. Taylor, in a paper by Putnam and Taylor *(loc. cit.)*, has given a most classic description of the pathologic histology of the spinal cord in pernicious anæmia and other conditions which have been referred to. I would, therefore, refer those, who desire an extended description of the cord in these conditions, to that paper. For the reasons given by Taylor I have adopted the term diffuse degeneration of the spinal cord for the lesions under discussion. It is sufficient to say here that all reporters describe the degenerative process as apparently of two kinds, one of which seems to be more acute, consisting apparently of a process in which the nerve sheath becomes swollen, stretched, and filled with debris, consisting chiefly of fat droplets. This process is not usually attended with an increase of neuroglia, and in

consequence the degenerated nerve sheath and fibre and its products are absorbed, leaving a vacuolated appearance, in which sometimes a normal or relatively normal axon may be seen. This process is beautifully shown in Case 1, in most of the figures, and in many figures of Cases 2 and 3.

Another more chronic process occurs, apparently coincidentally, in which there is an associated glia increase, with sclerosis, although contraction does not occur apparently, as in some other sclerotic processes of the cord. These two processes are often seen side by side, the one apparently chronic, with a tendency to neuroglia proliferation; the other acute, and sometimes very extensive. The more active process may explain the sudden occurrence of severe symptoms which sometimes takes place in the course of the disease. Bastianelli has maintained that the sclerosis in pernicious anæmia is not as compact as in the diffuse degeneration due to other conditions. In Case 1 of my series this sclerotic process was very dense in the upper thoracic and lower cervical regions in the column of Goll (see figure), and in the other two cases that were typical examples of pernicious anæmia, in which both processes were present, the sclerosis at some levels was very dense.

Causes of Spinal Cord Degeneration.—The lesion in the spinal cord has been attributed by different writers to the following causes:

1. To multiple hemorrhages, such as may occur under the skin and elsewhere in pernicious anæmia. This is not supported by the conditions found, for the reason that in many instances the lesion is diffuse, and does not extend upward from a possible hemorrhagic focus of degeneration in one tract, and downward in another, as it would necessarily do if the degeneration started from such a hemorrhagic point.

2. To thickening of the walls of the blood vessels. This cause of the degeneration has been especially advanced

by Nonne and by Minnich. They have reported hyaline degeneration and thickening of the walls of the blood vessels, with an exudate into the perivascular space. Most reporters have not found any marked thickening of the blood vessels, certainly not sufficient to be the cause of the degenerative process; and, furthermore, the process is usually not as diffuse and not as often focal as it would be if the primary cause of the disease was due to an arteriosclerosis. With thickening of the walls of the blood vessels, as found, it is just as probable that it is a part of the general sclerotic process.

3. To acute myelitis, which has been ascribed by Leyden as the cause of the disease in its earliest stage. No round-cell infiltration is found, and the distribution is too irregular, the spinal gray practically always free, which would rationally exclude myelitis.

4. To a primary change in the gray matter of the cord, of the brain, and of the ganglia. As stated, Mott has reported two cases in which he found degeneration of the large cells of the cortex, but the majority of reporters have not found complete neuron degeneration, and in the great majority of cases the spinal ganglia, the brain cortex and the spinal gray have been found healthy.

5. To anæmia. Poverty of blood alone is probably not the cause, for the reason that similar lesions do not occur in grave secondary anæmias. So far as any experimental work regarding secondary anæmias is concerned, chronic degenerative cord lesions have not been produced, and Mott states that chronic spinal cord changes cannot be produced experimentally. Furthermore, Bastianelli states that he and his colleagues have never found changes in the spinal cord in many cases of grave secondary anæmias due to malaria, although careful examinations, both macroscopic and microscopic, of the cord and brain, have been made.

6. To a toxic agent. The consensus of opinion is that

the nervous lesions are due to a toxic agent, which is also responsible for the anæmia when it is present. The fact that the lesion does not involve, as a rule, a whole neuron or system; that the trophic cell is usually not disturbed in either the cortex, the spinal gray, or the posterior ganglion, and that the ultimate primary change is one of degeneration of the nerve fibre itself, speaks for the effect of some blood-circulating toxin. The fact, too, that the brunt of the lesion occurs in that part of the tracts involved farthest removed from the trophic center, where the nerve fibre is the least protected by its nutritive cell, is evidence of a toxic cause.

The Degenerative Processes of the Spinal Cord in Other Conditions than Pernicious Anæmia.—As stated above, diffuse spinal cord lesions similar to those of pernicious anæmia have been reported by Putnam, by Dana, by Russell, Batten and Collier, and by others. Indeed, the now classic ataxic paraplegia of Gowers does not differ essentially, as one notes his description of it, from the reported condition of the cord of many of the writers upon the subject of this paper. Gowers says that the posterior and lateral columns are both affected, but usually less intensely in the lumbar than in the thoracic cord. He notes that the root zone is not as often affected as in tabes; that the degeneration does not reach the posterior periphery of the cord, and that the degeneration of the lateral columns is not often systemic in character. In many cases reported by Putnam in his first paper, and by Dana, the degenerative process is so similar to that described, especially in the severer degrees of degeneration found in pernicious anæmia, that one could rationally say the processes were the same. Putnam noted degeneration in the spinal gray in some cases in his first paper, but in his second paper with Taylor emphasis is placed upon the fact that the gray matter, the spinal ganglion, and the superficial nerves are usually not

involved. In thirty-eight of Putnam and Taylor's fifty cases, six only were noted to have pernicious anæmia. Of the nine cases reported by Russell, Batten and Collier, two only had pernicious anæmia, and in the nine cases reported by Burr, two did not suffer from pernicious anæmia. In all of these cases the spinal cord changes were practically the same, differing only in the degree of the spinal cord involvement. Indeed, Russell, Batten and Collier divide the cases clinically into three stages. The first, in which the patient is paræsthetic, with possibly slight ataxia and spasticity; the second stage, in which is added an increased ataxia and spasticity, with increased knee jerks, and slight paraplegia; and a third stage of flaccid paraplegia, with absent knee jerks, loss of sphincter control, bed-sores, emaciation, etc. A patient dying in the first stage of the disease would show usually less marked degenerative lesions than would one of the second or of the third stage.

This division of the cases clinically, while practical for study, is one which cannot be fully maintained, for the reason that all cases do not pass to the stage of even spastic paraplegia. Case 1 of my series died with symptoms only of subjective paræsthesia, and yet the spinal cord changes were sufficiently marked to have produced marked spastic, if not paraplegic, symptoms. Case 3, too, did not show flaccid paraplegia at the time of death, although the spinal cord changes, as shown by the figures, were very marked.

Spinal cord degenerative changes have been noted also in other conditions than those reported above. Williamson reports two cases of diabetes mellitus in which there was sclerosis of the nerve fibres in the posterior columns, especially in the column of Goll, with a moderate increase of neuroglia. Nonne quotes Tuczek on sclerosis of the posterior and lateral tracts in pellagra poisoning. This occurs as a symmetrical degeneration, and includes usually the column of Clarke, but the membranes of the cord remain free. It

is a chronic disease, with remissions and exacerbations, and may be mistaken for amyotrophic lateral sclerosis, ataxic paraplegia, or hysteria. Nonne reports two cases of leukæmia in which there was sclerosis in the middle column of Goll and Burdach, greatest in the dorso-cervical region but for a few small areas in the lateral and in the right pyramidal tract. There was no change in the spinal gray, in Clarke's column, in the root zone, in Lissauer's tract, or in the blood vessels. Nonne calls atention to the fact that this differs from the changes of pernicious anæmia, in that in the latter he has always found disease of the vessels of the spinal cord. Nonne also quotes Walter Müller, who reports a case of leukæmia, with spinal cord changes similar to those described by him. E. Bloch and H. Hirschfield report a case of myelogenous leukæmia, in which the cord showed moderate sclerosis of the posterior and lateral columns, greatest in the cervical region, and diminished from above downward. They quote a case by Kast of myelogenous leukæmia in which there was some sclerosis in the medulla, but none in the cord. They also quote Minnich, who reported a case of myelogenous leukæmia, with cord changes of a degenerative character, which he attributed to œdema consequent upon the anæmia. Similar cord changes have been found in diphtheria, lathyrism, and in certain metallic poisoning, like phosphorus, lead and arsenic.

It must be evident to one who has had experience with pernicious anæmia, and who is familiar with the literature of the subject of diffuse degeneration of the spinal cord as it occurs in pernicious anæmia and other conditions, that there is a marked similarity of the contributory causes of pernicious anæmia and of the conditions which are mentioned as causative in the production of the spinal cord lesions of other conditions. For example, in Putnam's first report he notes the age, the sex, the nutritional condition, the general feebleness and lack of vigor, the pre-existence of

infectious disease, like la grippe and typhoid, the developmental fault, the neuropathic history of epilepsy, goitre, etc., and the existence of chronic gastro-intestinal disease as important factors in his cases. These very conditions are prominent elements in many of the cases of pernicious anæmia. The consensus of opinion, as stated, is that pernicious anæmia is due to a hæmolytic toxin, and it is the universal belief, too, that the degenerative changes of the spinal cord in these different conditions are due to a toxin. It is suggestive, therefore, that possibly a toxin, which has its origin perhaps in the way suggested by Adami, may be one which may have in certain individuals a predilection for hæmolysis; in another for degeneration and sclerosis of tissue which, by election, affects the liver in one case, the kidney in another, and certain parts of the spinal cord in another. We recognize tabes as a parasyphilitic disease, and believe that the toxin associated with syphilis has a predilection for the posterior columns. It affects but a small percentage of syphilitic cases. Our want of knowledge of the nature of the toxin or toxins makes a discussion of this part of the subject purely theoretic, and of no value, although perhaps interesting. It is clear, however, that the future study of these degenerative processess must lie in the direction of an attempt at discovering the source and nature of the probable toxin, not only of pernicious anæmia, but of the degenerative changes of the cord in other conditions.

CONCLUSIONS.

The conclusions which one may draw from a study of the subject are:

1. That there is a well-established relation of diffuse cord degeneration with pernicious anæmia.

2. It seems probable that the hæmolysis and the cord changes are due to the same toxin.

3. While the source of the toxin is unknown, the fact

that gastro-intestinal disturbance is so common in the disease would lead one to suppose that it is of intestinal origin.

4. The diffuse degenerations of the spinal cord which occur in conditions without pernicious anæmia do not appear to differ essentially from those of pernicious anæmia.

5. It is possible that a common blood circulating poison exists which may expend its force upon the blood in one individual, upon the nervous apparatus in another, and coincidently upon the blood and spinal cord in others.

NOTE—I desire to here express my appreciation of the aid afforded me by Professor L. F. Barker of the University of Chicago, in examining sections of the spinal cord; to Dr. J. F. Smith of the House Staff of the Presbyterian Hospital, and to Drs. Robinson and Bassoe for aid in preparing sections of the cord.

APPENDIX.

CASE 4. — Female; aged 34; American; married; has borne one child. Had a miscarriage at one time, but cause unknown. Patient comes from a neurotic family, but there is no history of any grave inherited disease, either nervous, malignant, or tubercular. There is an uncertain history of acquired syphilis.

The patient was first seen May, 1898. She complained of a feeling of weakness, which had begun about a year previously. Her endurance was very much less. There was some gastro-intestinal disturbance, but apparently this was of no importance and depended somewhat upon the nervous condition of the patient. She had had considerable worry and fret concerning domestic affairs, and was neurasthenic.

A blood examination revealed 2,800,000 red cells; 40 per cent. hæmoglobin, and 8,000 white cells per cubic millimeter. This represented a rather severe secondary anæmia. No plasmodia malariæ were found. The spleen was palpable. There was a slight eczematous eruption in the axilla and about the pubic region. The urine was light in

weight, but contained no abnormal ingredients. There were no subjective or objective nervous disturbances referable to the spinal cord. No definite diagnosis was made. Patient was placed upon arsenic and iron and general hygienic treatment.

She was seen again in the fall in 1898, and at that time complained of weakness of the lower extremities, associated with paræsthesia. There was tenderness over the anterior crural nerve and some branches of the sciatic. The knee jerks were present, but not increased. Patient had good control of the bladder and the bowels. Tactile, temperature, and pain sense were objectively normal. The anæmia was of practically the same grade as when seen in the preceding May. The patient was not seen again until August, 1899, when she presented marked evidences of paraparesis, associated with paræsthesia of the skin of the lower extremities. There was marked diminution of the tactile, temperature, and pain sense, and partial loss of control of the bladder. Marked ataxia of station and locomotion was present. Knee jerk was entirely lost; the Argyll-Robertson pupil was not present; the fundi were normal. The temperature was subfebrile, ranging to about 100° in the evening; pulse from 90 to 100. From the middle of August the patient grew worse, with rapidly diminishing power in the lower extremities, until complete flaccid paraplegia resulted, with loss of control of both the bladder and bowels.

On August 31, a blood count showed 34 per cent. hæmoglobin; 1,010,000 red cells; 6,000 white cells, with 228 nucleated cells per cubic millimeter, of which 189 were megaloblasts. Poikilocytosis and polychromatophilia were present. The paralysis extended upwards, involving successive segments of the cord, with the corresponding nerves, and she died in September, 1899, with involvement apparently of the respiratory and cardiac centers.

Unfortunately, a post mortem examination could not be obtained. But the history is one of chatacteristic spinal cord degeneration occurring in pernicious anæmia, and the case would naturally fall into the second group of Bastianelli's cases. The anæmia for the first year or more was certainly not of a pernicious type, although it was grave. But preceding death the blood find was typical of pernicious anæmia. Like other cases reported, the progress was very rapid, when the nervous symptoms became marked by the paraparesis, and in the later stage the whole appearance of the patient, together with the blood find, was that of a rapidly progressing pernicious anæmia, with paralysis.

Case 5. — Male; aged 56; laborer; was first seen and examined on December 2, 1901. The family history was good, and the patient's habits were unexceptional. He dedenied venereal disease. Had always been well, and worked hard up to the beginning of his last illness. For eighteen months he stated he had grown gradually weaker in all the muscles of the body, but especially in the lower extremities. Ten months before he had noticed tingling and numbness in the left hand, and though he had frozen the fingers, the right hand soon became affected in the same way. The disturbance did not extend above the wrist. Five months ago the same sensation of numbness and tingling in the feet, legs and thighs, and a constricting band was felt about the waist. In August, 1901, Dr. F. A. Stephens, of Belmond, Iowa, made an examination of the blood, which showed 2,000,000 red cells, and 65 per cent. of hæmoglobin. About the middle of December the Doctor found 4,120,000 red cells, 85 per cent. hæmoglobin, and 7,500 white cells. Color index in both of these examinations was higher than normal. The patient has been taking arsenic for four months. For the last two or three months the patient has noticed some difficulty in walking. His gait is uncertain, but he can control it by

close attention to the feet, and he has never fallen. He has no more difficulty in getting about in the dark than in the light. He has good control of the sphincters of the bladder and bowels, but there is, he says, not the normal feeling during micturition and defecation. There has been no disturbance of the gastro-intestinal tract. His weight is 135 pounds, the average normal. Examination showed a man of rather aged appearance, with a slightly icteroid pallor. The gastro-intestinal tract negative; the chest negative, excepting for a faint systolic blow at the apex, which was not transmitted. The liver dullness began above the fifth interspace in the mamillary line, and extended two fingers' breadth below the ribs, where the edge could be felt distinctly indurated, sharp, and somewhat tender. The spleen was not palpable. There was rather marked pigmentation in the axillæ, perineum, scrotum, and along the vertebræ. There was no increased pigmentation of the face or of the hands, and the nipples were not pigmented. The skin showed an urticarial rash when irritated by scratching. There was hyperæsthesia, and pain from the waist downward. Temperature and touch sense appeared normal. Muscular sense normal; a slightly ataxic and spastic gait; the knee and plantar reflexes increased, and ankle clonus was present. Cremasteric reflex was diminished. The eye reflexes and eye grounds were normal; urine normal. Blood count showed 3,600,000 reds; 84 per cent. of hæmoglobin; 4,500 whites, and a few normoblasts and megaloblasts. The color index in this examination was 1.3. The nervous symptoms in this patient steadily progressed, and, when last heard from, he was almost helpless with a spastic paraplegia, and the evidences of pernicious anæmia still very marked.

Case 6. — Male; aged 62; merchant; resident of Wyoming; appeared February 28, 1902. He had typhoid fever during the war of the Rebellion. He went

to Wyoming twenty-five years ago because of asthma. In September, 1901, his appetite failed for meats, and it was noticed that he was pale. He visited West Baden, Indiana, and his general condition seemed to improve. He still complained of anorexia, and of general weakness. About January 1, 1902, he noticed constant numbness in the fingers and toes; at that time he had considerable eructation of gas, and his bowels became constipated. He had slight shortness of breath.

Examination showed an icteroid pallor; mucous membranes pale; lungs negative. Outline of heart dulness normal; a soft, systolic blowing murmur heard at the apex; no venous hum in the neck; no petechiæ of the skin or of the mucous membranes; the skin sensations subjectively normal. Knee jerks increased; slight ankle clonus; slight ataxia, and slight spasticity when walking. The eye grounds were normal. Liver palpable under the border of the ribs; abdomen flat; blood: red cells, 1,490,000; hæmoglobin, 47 per cent.; whites, 6,700. Differential count of whites: Polymorphonuclears, 69 per cent.; small mononuclears, 20 per cent.; large mononuclears, 9 per cent., and basophiles, 1 per cent. Both normoblasts and megaloblasts were found. The stomach contents, after a test meal, disclosed achylia gastrica.

The patient was placed in the Presbyterian Hospital, where many examinations of his blood and his stomach contents confirmed the findings of the first examination, and established definitely the diagnosis of pernicious anæmia. The urine showed nothing abnormal after repeated examinations. The nervous phenomena increased in severity. The patient complained daily of discomfort, of numbness, tingling, and usually of severe burning in the feet, and of numbness and tingling in both hands. The ataxia and spasticity increased; knee jerks and ankle clonus gradually increased, although during that time his blood condi-

tion improved over 50 per cent., and the patient finally felt so much better that he left for his home in Wyoming on May 13th. There can be no question that this case will relapse, and that the spinal cord phenomena and lesions will grow rapidly worse.

CASE 7.—Female; aged 30; has borne two children; German by birth. Family history is entirely negative. The patient was robust, with rosy cheeks, when a girl, and was also well until after her first child was born, eight years ago. There was general œdema of the body eight months after the first childbirth, which lasted two or three months. Then, there was apparent recovery, and a second child was born six years ago. General weakness followed the second childbirth, so that the patient was hardly able to work. Her present trouble began about three years ago, when she felt very weak, and it was noticed that she was very pale. She had nausea and vomiting, with frequent attacks of diarrhœa, but without pain. The nausea and vomiting persist until the present time. Her friends noticed that her skin was not only pale, but slightly yellowish. One year ago the patient noticed that the finger-tips of both hands were numb. A girdle sensation appeared about the waist and lower abdomen six months ago. Numbness of the feet occurred about the same time. Gradually she became weaker, the lower extremities finally becoming so weak that she was unable to walk, and for nine weeks she has been confined to her bed. She has had some fever during the past six months, which has been irregular in its occurrence, lasting three or four days at a time. She says that three years ago her tongue and gums were sore; that the upper teeth rapidly decayed, and were finally all extracted. The lower teeth remained good.

She was admitted to the Presbyterian Hospital on April 30, 1902, complaining of great weakness. There was a sense of a girdle about the waist and abdomen, and a band-

like feeling about the calves of both legs. She had no appetite, and there were nausea and vomiting frequently after an attempt to take a meal or any liquid food. The bowels moved from three to five times a day; she was very pale; icteroid hue; mucous membrane of the mouth pale; lower teeth in good condition. She wore an upper plate, and the gums were healthy. The chest was negative, ex-excepting for a systolic bruit, which was heard loudest over the pulmonary area, but could be heard over the whole precordia. The abdomen was negative. The pelvis was negative. The patient was unable to stand because of weakness of the lower extremities. There was no alteration objectively of sensation; the knee jerks were greatly increased; ankle clonus was present. The patient had good control of the bowels and of the bladder. The eye reflex was normal to accommodation and light. The eye grounds were normal. Urine contained nothing abnormal, but was scant. On May 1st the blood showed 1,614,000 reds; 33 per cent. of hæmoglobin, and 5,600 whites. A differential count gave polymorphonuclears, 21.3 per cent.; myelocytes, 1.4 per cent.; small mononuclears, 74 per cent., and large mononuclears, 3 per cent., and eosinophiles, .3 of 1 per cent. A large number of normoblasts and megaloblasts were present. The stomach contents, after a meal, showed the absence of hydrochloric acid, and no bacteria in the smears.

This case, from the history, was clearly one of pernicious anæmia of more than one year's standing, which began with severe gastro-intestinal disturbance, while the nervous phenomena occurred much later, were progressive, and reached the stage of spastic paraparesis at the time of her admission to the hospital. During May the patient improved steadily in her general condition, with simple abundant food, iron and arsenic. The blood state at the last examination showed an improvement of 10 per cent. in

red cells, and 5 per cent. in hæmoglobin. Nausea and vomiting had ceased entirely, and the patient was able to stand with the aid of a chair and to take a few steps. The spastic condition of ataxia and paresis, while less, was still very marked. The patient is still under observation.

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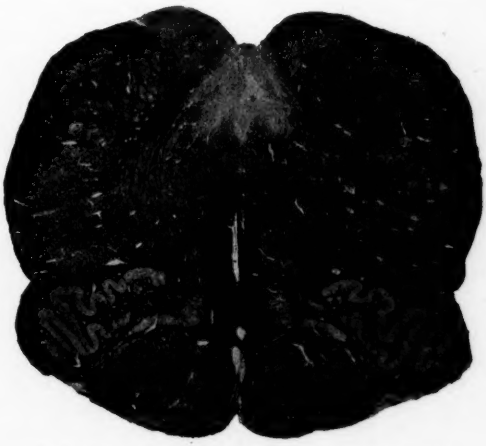
CASE 1, FIG. 1.—(Weigert-Pal.)
Fourth Lumbar.
Degeneration of crossed pyramidal tracts.
Remainder of cord normal.

CASE 1, FIG. 2.—(Weigert-Pal.)
Second lumbar.
Crossed pyramidal tracts degenerated.
Remainder of cord normal.

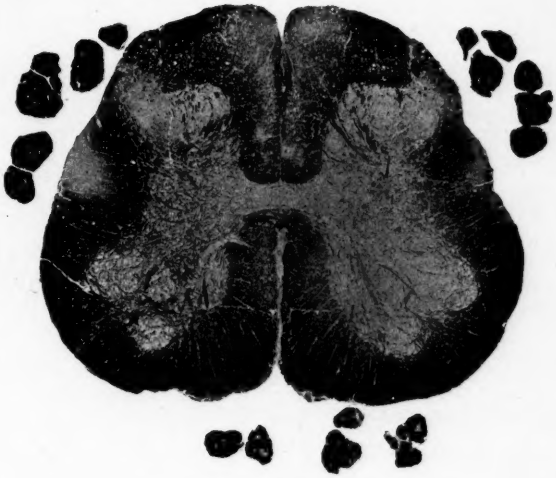
CASE 1, FIG. 3.—(Weigert-Pal.)
Middle Thoracic.
Goll moderately degenerated.
Lateral tracts more extensively involved.

CASE 1, FIG. 4.—(Weigert-Pal.)
About fifth cervical.
Column of Goll degenerated and sclerosed in middle. Burdach, slightly degenerated in proximity of sclerosis of Goll.
Lateral tracts extensively involved.
Remainder of cord normal.

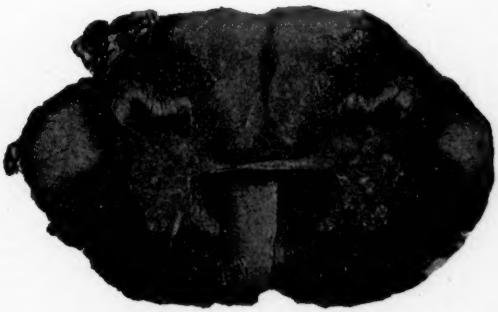
CASE 1, FIG. 5.—(Wigert-Pal) × 8.
Near junction of cord and medulla.
Columns of Goll extensively degenerated, otherwise the section is normal.



CASE 1, FIG. 6.—Medulla, normal.—(Weigert-Pal.)



CASE 2, FIG. 1.—(Weigert-Pal.)
Fourth lumbar.
Post. column degenerated. Oval bundle of Fleschsig normal.
Lateral tracts unequally degenerated.
Remainder of cord and nerve roots normal.



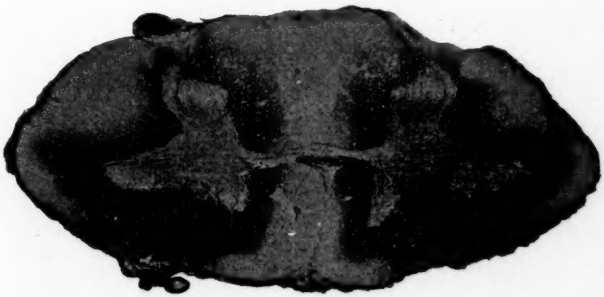
CASE 2, FIG. 2.—(Weigert-Pal.)
About level of twelfth thoracic.
Post. degeneration more extensive and sclerosed.
Lateral more extensively and almost equally degenerated.
One direct pyramidal tract extensively degenerated.

CASE 2, FIG. 3.—(Weigert-Pal.)

Mid. Thoracic level.

Degeneration very extensive—involving the whole periphery of the cord.

The gray matter of cord, the post. root zone and Lissauer practically normal.



CASE 2, FIG. 4.—(Weigert-Pal.)

Level of eighth cervical.

Posterior columns, and the pyramidal, direct cerebellar and Gower's tracts in the lateral columns and the direct pyramidal tracts extensively degenerated and sclerosed.

The gray matter, Clark's nucleus, Lissauer and the root zone free.

CASE 2, FIG. 5.—(Weigert-Pal.)

Level of junction of cord and medulla.

Column of Goll entirely degenerated, except at gray commissure.

The medial and posterior parts of the columns of Burdach are completely degenerated, leaving the lateral portion normal.

Both direct cerebellar tracts are degenerated.

One pyramid is much degenerated; the other very slightly changed.

The spinal tract of the trigeminus is unaltered.

CASE 2, FIG. 6.—(Weigert-Pal.)

Level of inferior medulla.

The upper extremity of column of Goll is completely degenerated.

The column of Burdach, excepting a small area between the nucleus of Burdach and the posterior horn, is degenerated.

The direct cerebellar tracts are degenerated.

One pyramid is much, the other only very slightly altered.

Other portions of section, normal.

CASE 2, FIG. 7.—(Weigert-Pal.)
The lower medulla.
Moderate diffuse degeneration of pyramids.
Columns of Burdach degenerated.
Direct cerebellar tracts degenerated.

CASE 2, FIG. 8.—(Weigert-Pal.)
The mid. medulla normal except for very slight degeneration of the pyramids.

CASE 2, FIG. 9.—(Weigert-Pal.)
Section of lower pons.
Extensive and intense degeneration of both pyramids—
one more than the other.
Otherwise normal tissue.

CASE 2, FIG. 10.—(Weigert-Pal.)
Section of upper pons, normal.
The ventrally located white border may be an artefact

CASE 3, FIG. 1.—Weigert-Pal.)

Section at level of first lumbar.
Degeneration of Burdach in middle zone chiefly.
Root zone and Lissauer free.
Crossed pyramidal tracts degenerated.
Spinal gray, remainder of white, cord and spinal roots normal.

CASE 3, FIG. 2.—(Weigert-Pal.)

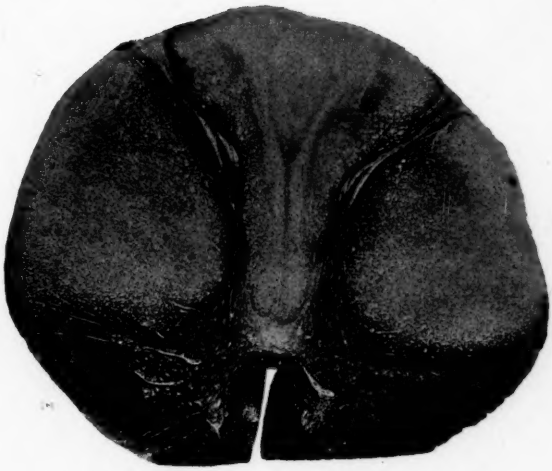
Level of second thoracic.

Extensive degeneration of columns of Goll and Burdach, excepting the border of white at gray commissure and post. horns.

The lateral tracts invaded extensively.

The direct pyramidal tracts unequally degenerated in a small area.

The gray matter, the root zone, Lissauer and nucleus of Clark remain practically free.



CASE 3, FIG. 3.—(Weigert-Pal.)

Level of fourth cervical.

Extensive degeneration with marked sclerosis of Goll. Extensive, but less sclerotic degeneration of Burdach. A narrow band of almost normal white tissue persists at the gray commissure and bordering the post. horns.

The root zone and Lissauer are practically free.

Extensive degeneration of the lateral tracts, old and recent, as indicated by the dense whiteness and the cribriform appearance, respectively.

Slight degeneration in one direct pyramidal tract.

The gray matter, cells in ant. horns, Clark's nucleus, normal.

ARTICLE III.

DEMONSTRATION OF A MODEL OF THE THORACIC AND ABDOMINAL VISCERA PREPARED FROM A HUMAN SUBJECT HARDENED IN FORMALIN.

By JOHN WARREN, M.D.
OF BOSTON.

JUNE 11, 1902.

DEMONSTRATION OF A MODEL OF THE THORACIC AND ABDOMINAL VISCERA PREPARED FROM A HUMAN SUBJECT HARDENED IN FORMALIN.

THIS model was made during the winter of 1902 in the Anatomical Department of the Harvard Medical School. A few years ago, a model of the abdominal viscera was made under the direction of Professor Dwight by this same method. This model was enlarged three times on a linear scale, and was intended for use in the lecture room; later four smaller models of actual life size were made for section teaching. These models were so successful that we attempted this time to make a model of the entire trunk with the thoracic and abdominal visera in situ.

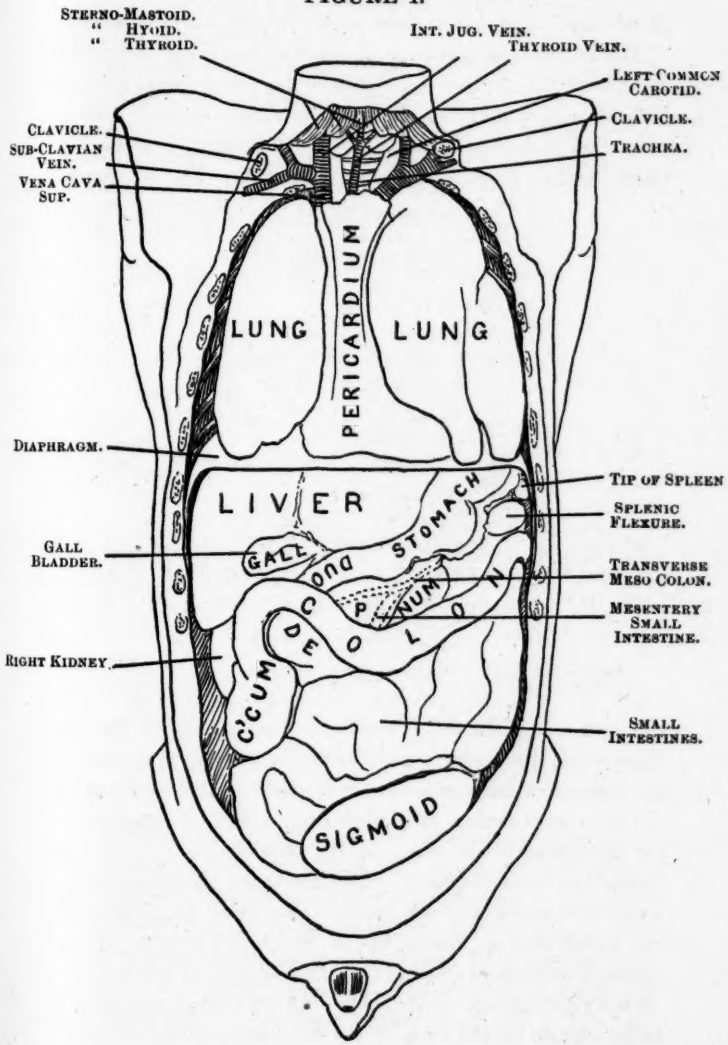
The method of preparation was as follows. The best subject then on hand was selected. It was an adult male about 65 years of age, muscular and apparently well nourished. The body was hardened by successive injections of formalin, which was allowed to move very gradually into the right femoral artery; about four and one-half quarts were used in four strengths of 10, 25, 35, and 50 per cent. respectively. This method of hardening subjects by formalin is a well-known one, and has been used extensively by Prof. Cunningham of Dublin and Prof. His of Leipzig. It was also used by Prof. Dwight to prepare the models made here previously. It is the best method of obtaining the exact shape and position of the various organs, and their relations to each other, and is especially useful in the study

of variations in the size and shape of the abdominal organs.

The subject was allowed to harden about three weeks and then the larger vessels were filled with the usual wax mass. The entire anterior wall of the trunk was thus removed and a cast in plaster taken of all the viscera in situ. The lungs were removed, a cast of these organs made, and then a cast of the pleural cavities with the pericardium in place. Next the pericardium was removed, the great vessels in the superior mediastinum dissected out, and a cast of these structures made. The heart and structures in the posterior mediastium were then treated in the same manner. The diaphragm was then cut at the sides and laid back to enable us to make a cast of the upper surface of stomach, spleen and liver. The various abdominal organs were then removed in succession, and treated in the same manner as the thoraic viscera. From these casts the organs were modeled in blotting paper with the exception of the spleen and kidneys, which were made in plaster. These paper models are much better than plaster ones, as they are very light and durable, and especially useful in a model which is to be handled to any extent. Plaster models are unwieldy and tend to chip and break. The casts and modelling were done by Mr. J. H. Emerton of Boston, who made the other models here, and this success has been largely due to his skill.

Fig. I shows the whole model with all the viscera in place. The lungs were very difficult to model, as they did not harden well, and the right lung was much contracted, and its fissures entirely obliterated by old inflammatory adhesions. A portion of the posterior lobes behind had to be removed in order to enable us to remove and replace the models easily. Therefore we have contented ourselves with a rough model only of these organs, chiefly to show their relations in front to the pericardium. The anterior edge of the diaphragm has been cut away for about two

FIGURE I.

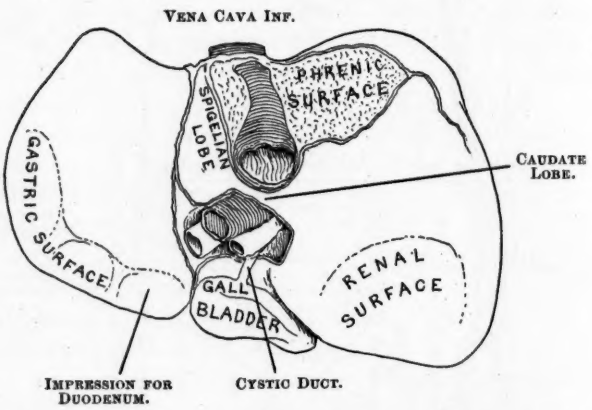

STERNO-MASTOID.
" HYOID.
" THYROID.
INT. JUG. VEIN.
THYROID VEIN.
LEFT COMMON CAROTID.
CLAVICLE.
CLAVICLE.
SUB-CLAVIAN VEIN.
TRACHEA.
VENA CAVA SUP.
LUNG
PERICARDIUM
LUNG
DIAPHRAGM.
TIP OF SPLEEN
LIVER
STOMACH
SPLENIC FLEXURE.
GALL BLADDER.
GALL
DUO
TRANSVERSE MESO COLON.
JEJUNUM
MESENTERY SMALL INTESTINE.
COLON
RIGHT KIDNEY.
CŒCUM
SMALL INTESTINES.
SIGMOID

inches, in order to expose the liver and stomach more completely and to enable us to remove these organs with ease.

The left lobe of the liver projects about three inches to the left of the median line; this is the usual limit of this lobe, though it may not uncommonly extend entirely across to the left abdominal wall. The gall bladder is distinctly large, though no gall stones were present. Fig. II shows

FIGURE II.

LIVER FROM BEHIND.



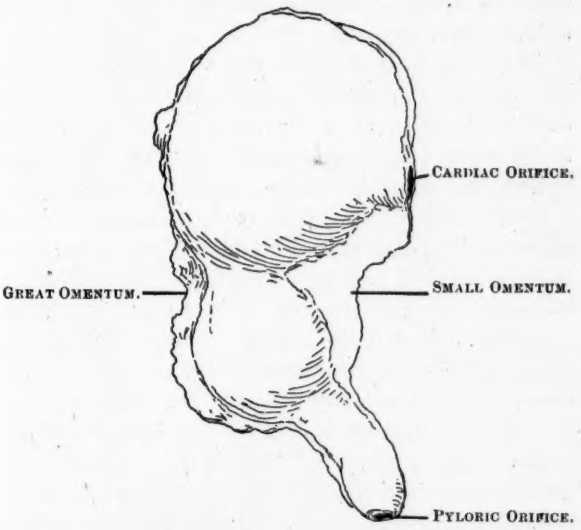
the liver from behind. The double lines represent the reflections of the peritoneum; the dotted area corresponds to the non-peritoneal area that is attached to the diaphragm. On the under surface of the left lobe is seen an impression for the stomach and also for the duodenum. This latter organ has been displaced somewhat to the left owing to the small and contracted stomach. The renal impression on the right lobe is placed much lower down and farther forward than usual; this is due, I think, to the low position of the right kidney (Fig. VI). Therefore there is quite a large surface on the back of the right lobe covered by peri

toneum, between this renal impression and the non-peritoneal area, where the renal impression should be, if the kidney was in its normal position.

The stomach is distinctly small, and in the model it has been slightly distended for convenience in making the cast. A distinct contraction is seen at about the middle of the greater curvature; this is much more distinct on the pos-

FIGURE III.

STOMACH FROM BEHIND.

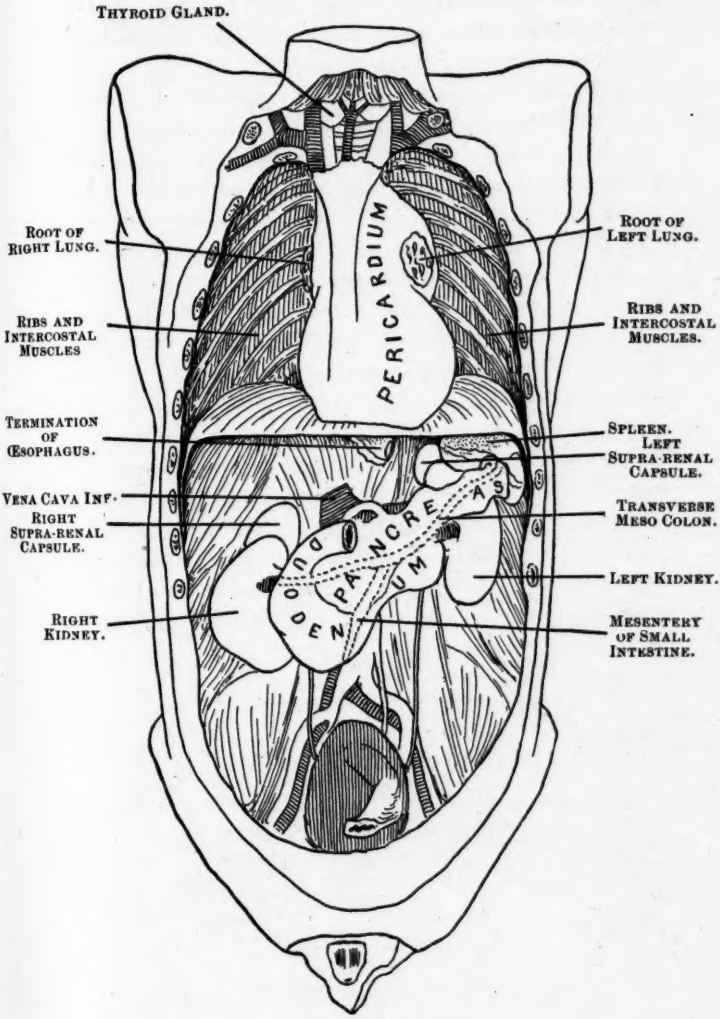


terior surface (Fig. III), and is probably an hour-glass contraction. Just below this, and about two inches from the pyloric orifice, is seen another contraction. This part of the stomach, known as the antrum pylori, is quite variable and may be double or even triple. Its tubular form is in this case quite striking. The pyloric orifice in this case lies in the median line, or if anything a bit to the left.

Usually this part of the stomach is more or less to the right of the median line, depending on the distention of the organ. The first portion of the duodenum is placed farther to the left than usual, and a distinct impression is seen for it on the wider surface of the left lobe of the liver (Fig. II). This part also runs almost directly to the right with but little of its usual backward direction. The first part of the duodenum and the pyloric end of the stomach are very movable, and their position is easily affected by the degree of distention or size of the stomach. The small size of that organ accounts, I think, for the position of the duodenum and pylorus in this case.

The cæcum is very small and very tightly contracted. The space seen between it and the abdominal wall is due to the fact that there was a large collection of fat here, which had to be removed in order to take the cast. No appendix could be found, though a most careful search was made. It is possible to have this organ absent, as was the case in a subject I saw this year in the dissecting room; but it is only fair to say that it may have been lost in the removal of the fat, which becomes very hard when treated with formalin, in spite of all care to the contrary. The ascending colon is short and is also very contracted, as is also the case with the transverse and descending colons. The latter is concealed by coils of small intestine, but the beginning of the sigmoid is quite dilated. (Fig. I.) The various coils of the small intestine have been represented in a rough model, as it was practically impossible to make an exact model that would fit. I do not expect, however, that they will be used in practical work with the model, and simply had them made to complete the abdominal contents. A part of the second portion of the duodenum is seen just below the hepatic flexure and the termination just above the transverse colon farther to the left. A portion of the pancreas (Fig. I. P) is seen within the curve of the duodenum; the dotted lines repre-

FIGURE IV.


THYROID GLAND.
ROOT OF RIGHT LUNG.
ROOT OF LEFT LUNG.
PERICARDIUM
RIBS AND INTERCOSTAL MUSCLES
RIBS AND INTERCOSTAL MUSCLES.
TERMINATION OF ŒSOPHAGUS.
SPLEEN.
LEFT SUPRA-RENAL CAPSULE.
VENA CAVA INF.
RIGHT SUPRA-RENAL CAPSULE.
PANCREAS
TRANSVERSE MESO COLON.
DUODENUM
LEFT KIDNEY.
RIGHT KIDNEY.
MESENTERY OF SMALL INTESTINE.

sent the attachments of the transverse mesocolon and mesentery of the small intestine. The tip of the spleen and the splenic flexure of the colon are seen on the left just below the greater curvature of the stomach.

In Fig. IV. the lungs, stomach, liver and intestines below the duodenum have been removed. The pericardium is seen in situ, the lines on its anterior surface representing the limits of the two pleural sacks. The pleura has not been represented and the ribs and intercostal muscles are seen in the two pleural cavities. The roots of the lungs are shown in section on either side of the pericardium. In the abdominal cavity the duodenum and pancreas are seen. The former is shown in its whole length, the second portion overlapping the right kidney and being crossed by the line of attachment of the transverse mesocolon, which extends along the body of the pancreas to the spleen. The third portion is crossed by the line of attachment of the mesentery of the small intestines. There is practically no fourth portion in this case, and though the duodenum with four portions is as a rule the more usual, still one with only three is not at all uncommon. The pancreas is well developed in this case and its tail is in close apposition with the spleen, there being a well marked

FIGURE V.

PANCREAS AND DUODENUM FROM BEHIND.

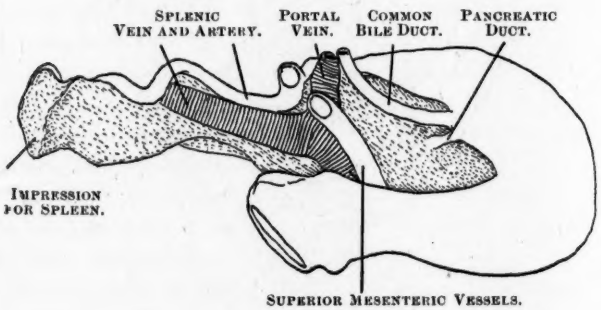
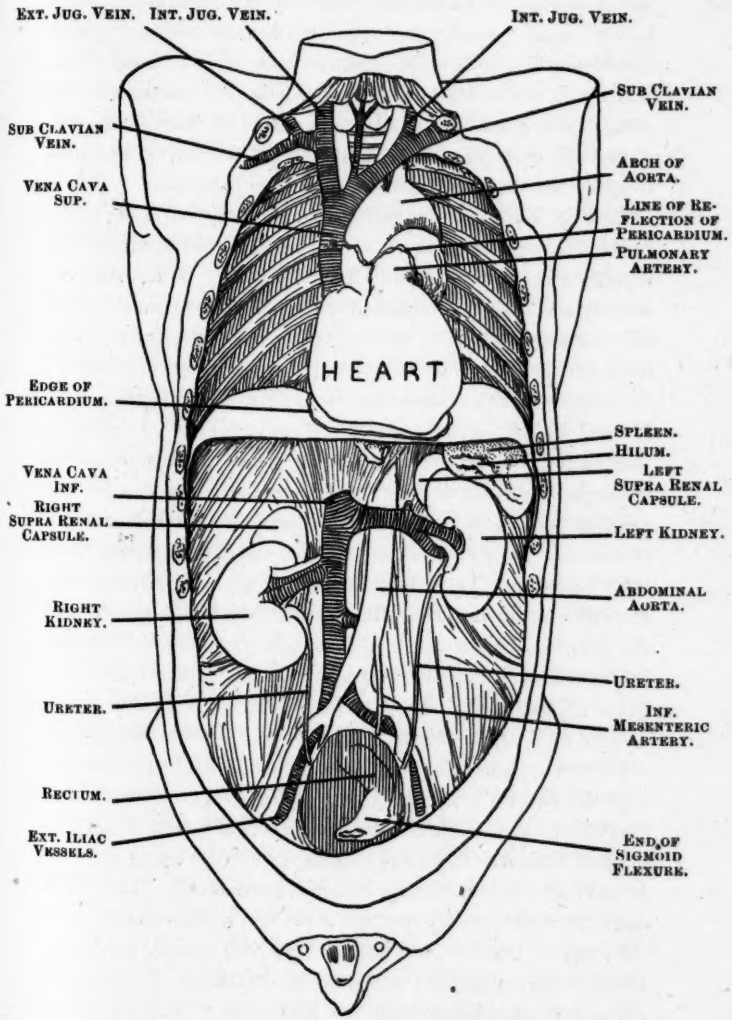


FIGURE VI.


EXT. JUG. VEIN.
INT. JUG. VEIN.
INT. JUG. VEIN.
SUB CLAVIAN VEIN.
SUB CLAVIAN VEIN.
ARCH OF AORTA.
VENA CAVA SUP.
LINE OF REFLECTION OF PERICARDIUM.
PULMONARY ARTERY.
HEART
EDGE OF PERICARDIUM.
SPLEEN.
HILUM.
VENA CAVA INF.
LEFT SUPRA RENAL CAPSULE.
RIGHT SUPRA RENAL CAPSULE.
LEFT KIDNEY.
ABDOMINAL AORTA.
RIGHT KIDNEY.
URETER.
URETER.
INF. MESENTERIC ARTERY.
RECTUM.
EXT. ILIAC VESSELS.
END OF SIGMOID FLEXURE.

impression there for that organ. Fig. V. shows the pancreas and duodenum from behind, these two organs being modeled in one piece. Along the upper border are seen the splenic vessels; passing down behind the body, but in front of the third part of the duodenum are the superior mesenteric vessels. The portal vein and common bile duct, with the pancreatic duct joining it at the second portion of the duodenum, are seen behind the head of the pancreas.

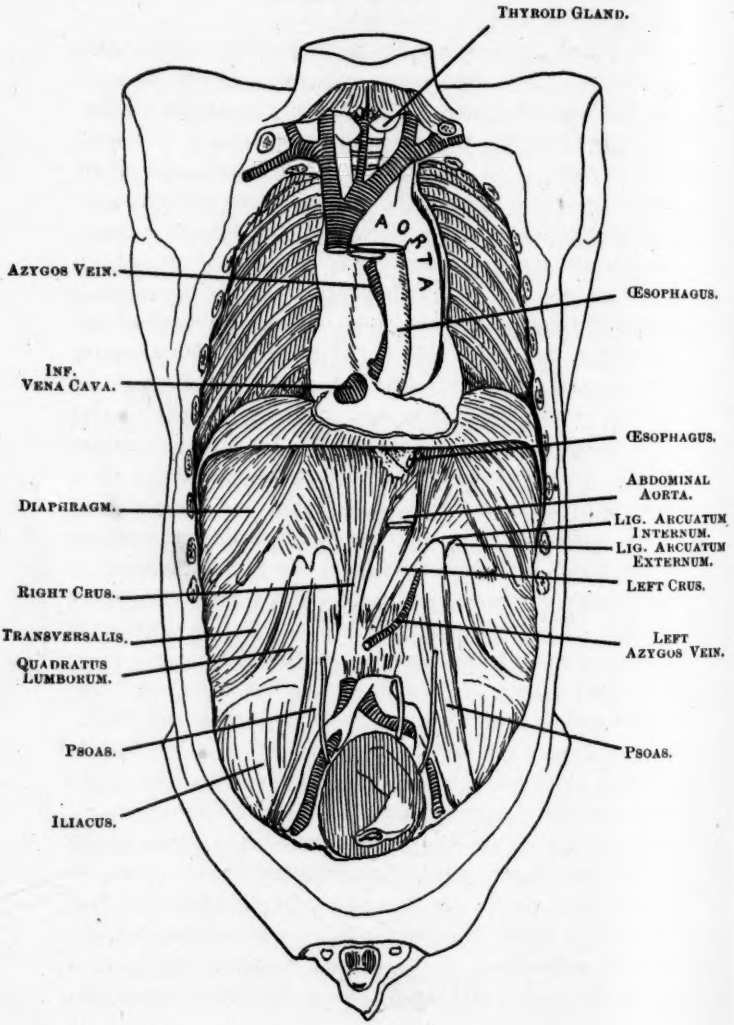
In Fig. VI. the heart and great vessels are exposed. The heart is large and the arch of the aorta rather dilated; this condition is probably due, partly to the age of the subject, and partly to some overdistention from the wax mass. For this latter reason, the beginning of the pulmonary artery does not lie so directly in front of the beginning of the aorta as usual. Across these vessels and the superior vena cava is seen the line of reflection of the pericardium. In front of the trachea is a large vein passing from the thyroid gland to the left innominate vein. Above the gland are the cut ends of the sterno hyoid, sterno thyroid and sterno mastoid muscles. In the abdomen, the duodenum and pancreas have been removed. The left kidney is in its usual position, but the right is displaced downwards and overhangs the brim of the pelvis. This is not due to pathological causes, I think, but is rather of congenital origin, as the right renal vein is given off two inches below the left. The suprarenal capsules are the only artificial part of this model. In the subject they were so very small, of such unusual shape, and the right one separated so far from its kidney, that it was impossible to reproduce them. Those for the suprarenal bodies of another subject were hardened and models made from them, in order to have all the abdominal organs represented. There is a distinct separation between the aorta and superior vena cava. The former bends to the left, and the iliac vessels, especially the left common iliac, are quite tortuous, as if the whole aorta was rather too long for the trunk. There were no

spinal curvatures present, however, to account for this condition, though perhaps over-distention by the wax mass may have had something to do with it.

In Fig. VII. the structures in the posterior mediastinum are shown. The aorta seems large, especially at the arch, and lies somewhat more to the left of the œsophagus than usual, probably for the reason just mentioned. In the abdomen, there is an admirable view of the muscles of the posterior adominal wall, the outlines of which were very distinct in the hardened subject. The diaphragm is shown with the two crura surrounding the beginning of the abdominal aorta, and the two arched ligaments, the ligamentum arcuatum internum and externum, arching over the psoas and quadratus lumborum. To the outer side of the quadratus lies the transversalis, and below this muscle the iliacus. To the left of the left crus is seen a left azygos vein passing from the inferior vena cava (Fig. VI.) to the left lumbar veins. In the pelvis the peritoneum has been left in place and the termination of the sigmoid flexure and the beginning of the rectum are seen. The external genitals were removed at the pubes, and the corpus spongiosum with the urethra, the corpora cavernosa, and the spermatic cords are seen in section. The trunk was made in paper, just as the various viscera were made, but some plaster has been placed along the spinal column to strengthen it and act as ballast. The whole model is quite light and easy to handle, and weighs with all the viscera in place fourteen and one half pounds.

These models have been found very useful in section teaching, as in the dissecting room the student often fails to get an exact idea of the shape and relations of the various organs, as they are usually soft, and impressions made on them by neighboring viscera are generally absent. There are several details about this model which are unusual or even abnormal,—the shape of the stomach especially, the position of the duodenum and right kidney, the size of the cæcum

FIGURE VII.


THYROID GLAND.
AORTA
AZYGOS VEIN.
ŒSOPHAGUS.
INF. VENA CAVA.
ŒSOPHAGUS.
ABDOMINAL AORTA.
DIAPHRAGM.
LIG. ARCUATUM INTERNUM.
LIG. ARCUATUM EXTERNUM.
RIGHT CRUS.
LEFT CRUS.
TRANSVERSALIS.
LEFT AZYGOS VEIN.
QUADRATUS LUMBORUM.
PSOAS.
PSOAS.
ILIACUS.

and colon. To be of any value, however, such a model must represent the exact anatomical conditions found in a given subject, and no attempt should be made to improve on nature. That is what has been done here with great care, with the exception of the supra-renal capsules, and I rather doubt the wisdom of what was done in that case. It is only by this careful representation that any correct idea can be obtained of the variations in the shape and relations of organs, and from a study of models prepared in this manner it may be found that there are wider variations within normal limits than one would suppose. Even if the conditions found here are not perfect from an anatomical point of view, they are certainly interesting for clinical study.

ARTICLE IV.

EMPYEMA IN CHILDREN.

By FREDERICK J. COTTON, M.D.
OF BOSTON.

READ JUNE 11, 1902.

EMPYEMA IN CHILDREN.

THIS paper is based on a series of cases of empyema in children; its deductions apply to children only.

Through the courtesy of Dr. Bradford, Dr. Burrell and others of the Children's Hospital staff, the writer has had an exceptional opportunity for the study of the treatment of empyema in children. The work was begun with the idea of studying the somewhat neglected "siphon" or negative pressure treatment. To this end special apparatus was devised; first, a hydrostatic apparatus in which the negative pressure exerted could be accurately estimated and regulated by means of a water-column, the chest cavity being connected by an air-tight joint with the air reservoir of the apparatus; and secondly, a form of valve dressing (of sheet gutta-percha) was contrived which rendered better service mechanically than the valves previously described by various writers.

This treatment by negative pressure, carried out on a number of patients, proved of definite value in the more chronic cases, where failure to heal was really due to non-expansion of the lung; these results will be reported elsewhere. In the acute cases the results were not especially brilliant, though better rather than worse than the average.

In the course of this work the writer became interested in studying the general course of the disease, its treatment and results, and it is from this point of view that this paper

has been written. This study has been greatly helped by the permission of Dr. Bradford and Dr. Burrell to make use of the Children's Hospital records since 1885. Many of these older cases were sent out for, and twenty-nine reported in person or by letter. A series of cases published in the Children's Hospital Reports, by Dr. Burrell, and an unpublished series by Dr. Goldthwait, kindly placed at the writer's disposal, gave data as to end-results in a number of other cases of this series. Four cases at the Infants' Hospital are also reckoned in, cases treated by the writer through the courtesy of the staff. The total is 180 cases — of these 45 were under the writer's care for a part or all of their course (21 of them for both operation and treatment) — in 86 the end results have been verified; there are in all satisfactory data in 146 cases, recent cases and those with defective records being omitted.

The data have been considered statistically, but the varying character of the cases renders statistics satisfactory only in the broader sense, and part of the conclusions reached are frankly deductions from a study of the cases in detail, which study cannot be reproduced here.

Age.—All cases were under 12 years — the youngest seven months. 51% were under five years. The average age was 4.9 years.

Etiology.—Of cases where data are given, 104 followed pneumonia, 9 followed a probable pneumonia; only 4 were definitely primary. In some few cases the pneumonia was secondary to exanthematous or other disease. So far as the records show, the pneumonia seems in all cases to have been of the lobar type.

Tuberculosis seems in this community to play a very slight rôle in causation of empyema in children. In the whole list there was no case of tuberculous empyema proved as such. Tuberculosis of the lung was proved by autopsy in one case, diagnosed clinically in five others, but in no case with demonstration of bacilli.

Cultures are recorded in 48 cases, 33 of which showed pure pneumococcus, 7 pure streptococcus, 1 (a double empyema) streptococcus on one side, pneumococcus on the other, 1 mixed pneumococci and staphylococci; 1 pneumococcus and various saprophytes, 2 pure staphylococcus, 1 saprophytes only, and 2 cultures were sterile.

This preponderance of pneumococcus infection, different though it is from the conditions in adults, is in conformity with other statistics for children published by Netter and others.

Date.—As to the date of occurrence there is a wide variation in the interval between the pneumonia and the empyema — from an imperceptible interval to many months. One case occurred a year after pneumonia, several at three to six months. The average interval is not quite seven weeks; the mean is about five weeks.

Physical Signs.—As to the physical signs little need be said. Even after short duration, large effusions are usually found. Encapsulated or sacculated effusions are the exception; only six are noted as such in the records. The usual type is that of a "total" empyema, flat to percussion from the third rib in front, from the scapular spine behind, with the heart considerably displaced.

The amount of effusion tolerated by children without cyanosis, dyspnœa, or obvious discomfort is relatively much more than is usual with adults.

Double empyema is recorded in but two cases; in both the second effusion was small, and was not operated on; both were autopsy cases.

Fifteen cases came in as empyema necessitatis or with a fistula from spontaneous opening. The point of rupture was posterior or axillary in four, in the classical position (2d to 5th space in front) in eight, unrecorded in two.

Spontaneous opening through the scar was not unusual in the recurrent cases.

OPERATION.—All cases were tapped as a preliminary. In some cases operation was deferred until it was evident that the effusion was not absorbing, and then performed.

In seven cases the effusion, usually small, subsided after tapping and no further operation was done.

In two cases operation showed little or no pus and drainage was not used or needed. In two cases where pus was present the attempt was made to let them close without drainage; these had to be drained later.

The operations done were—incision in the 7th or 8th space in the axilla, or subperiosteal resection of the 7th, 8th or 9th rib, about one inch of the rib being removed in the mid. or posterior axillary line. The choice of incision or excision was largely personal, save where in desperate cases incision gave the simpler operation. In either case, except as noted, tube drainage was used. All operations were under ether, or chloroform.

The writer prefers and has consistently used the excision operation, mainly because it gives room for digital exploration and for the shelling off and delivering of the massive fibrin clots which usually line the pleura. These, if left, disintegrate and about the 4th to the 10th day greatly interfere with proper drainage.

In two cases an opening was made into a presenting abscess and a counter opening made elsewhere for better drainage; in two other cases two openings were deliberately made. The cases showed no advantage in this procedure.

In no case was a double operation done for double empyema.

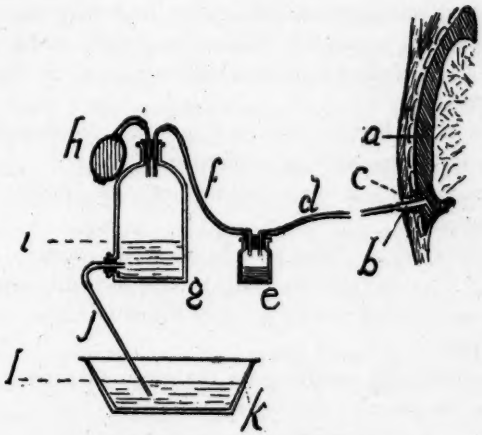
In two late chronic cases Estlander's operation was done. One died at two and a half, one at four weeks after this secondary operation.

Delorme's operation — decortication so-called — was not tried.

As to operative complications, temporary collapse is frequently recorded.

Hemorrhage on the table occurred in several cases, not from the intercostal artery but from the pleural surfaces; it always yielded to hot irrigagtion.

This complication seems to the writer the only occasion for irrigation at the time of operation. Irrigation *per se* is probably dangerous and hardly useful. It was however, practised in about half of the cases in our series, though relatively seldom since 1898, before which time it was the usual routine.



SUCTION-APPARATUS FOR EMPYEMA.

(a)—Chest cavity. (b)—Tube in opening. (c)—Gutta-percha sheet gummed down to make air-tight joint. (d)—Tube connecting chest-cavity with (e) Catch-bottle to receive discharge; connected by tube (f) with (g)—Reservoir bottle. (h)—Exhaust syringe. (j)—Tube connecting bottle (g) with water in the pan (k). The negative pressure is measured and maintained by the water-column the height of which is the difference of the levels of water at (i) and (l). The level (i) in the bottle (g) may be increased as desired through the pump (h).

AFTER-TREATMENT.—In a large proportion of cases, the simple drainage tube (with antiseptic dressings) was used.

Irrigation after the operation was more or less regularly used in 69 cases. Chlorinated soda and boric acid, weak

corrosive, salt solution, water, and in one case enzymol, were used. In a few cases irrigation by immersion (dependent upon respiration for the washing) was used. Of late years irrigation has been largely abandoned except in very foul cases.

The Cabot valve was used in many of the earlier cases. A similar valve of gutta-percha gummed on three edges by pasting it to the skin with chloroform, was devised and used by the writer. It is mechanically more efficient than the original form.

A suction-apparatus, devised by the writer (see p. 93), was used in a number of cases to apply a steady suction force (a partial vacuum accurately regulated) to aid in lung expansion.*

The Wolff bottles were used in a few convalescent cases to promote expansion by expiration.

Respiratory and other exercises were used freely for convalescents.

MORTALITY.—There were in the whole series 26 deaths, 14.4%. Of the patients under two the mortality was 37%; between two and five 20%; over five only 7.5%, a striking difference.

The mortality according to the infection gives (in cases where the infection was determined)

6 in 33 pneumococcus infections 18%
3 in 8 streptococcus infections 37%

twice as high for streptococcus, a contrast the more striking as the above list of deaths in pneumococcus infection includes one death from scarlatinal nephritis and one from diphtheria.

As to the causes of death:

Exhaustion, malnutrition and slow septic absorption account for 12,

* The negative pressure used with this apparatus was one sufficient to sustain a column of water of from eight to thirty inches, according to the case. The tension is obtained by an exhaust bulb, and remains constant for some hours without any attention.

Early collapse for 4 (there were no deaths on the table).
Persistence of pneumonia for 3.
Probable brain or other embolism for 4.
"Convulsions" 1.
Diphtheria 1.
Scarlatinal nephritis 1.

It is to be noted that the most frequent cause of death is "infection" or "exhaustion"—on the basis of records it is impossible to separate the two. In the cases personally observed there was undoubtedly infection, but the grade of infection of the discharging cavity seems to the writer of secondary importance. The cause of death in many at least of these cases is the profound disturbance of nutrition, due primarily to pneumonia, secondarily in some cases to delay in operating. In the writer's cases forced feeding, concentrated foods, rectal feeding, stimulants, tonics, etc., were used, usually without success. Some patients, especially the older children, show rallying power—the others, especially the little ones under two—grow gradually weaker and die.

Earlier operation may save some of them but the problem is essentially a medical one and I can offer no very helpful suggestions.

The other causes of death—embolism, recurring pneumonia, exanthemata, etc.—are so entirely accidental that there seems no way of guarding against them.

REEXPANSION AND HEALING.—Prompt and full re-expansion of the lung was frequently noted before the patient left the table; this proves that the lung was not tied down in these cases, but such expansion was found to have no definite relation to prognosis of life or healing.

Save that irrigation after operation seems to retard closure, the study of results *in relation to methods* is not very instructive.

The writer had hoped to achieve definite results with the use of suction-apparatus and valves. This has not been the case. It is possible to produce expansion of the lung in this way, and to get more than usually good drainage, but expansion does not, in itself, ensure healing—that also depends upon the nutritive condition, and in the cases with profoundly disturbed nutrition, which are common, the suction method yields only moderately good results. Its proper field is in the later cases where failure to re-expand is the prime factor in failure to heal. In five such cases it yielded most satisfactory results.

The statistical figures will, then, be given without reference to the special method used.

There are a number of cases in regard to which we have no data, others doing well in which too little time has elapsed for final records. Out of 105 cases with complete data,

19 were closed at six weeks,
42 were closed at three months,
27 closed later without operation,
14 closed only after secondary operation,
5 were not closed after years of fistula drainage.

Undoubtedly there may be other chronic empyemas among those not traced, but probably the figures are near the facts. It is noteworthy that empyemas apparently soundly healed may later break down and recur either as localized or extensive empyemata. There were ten cases in our series where this happened, and in two of them the recurrence was two years after healing.

No essential difference is to be made out as to time of closure according as the effusion is sterile or contains pneumococci, streptococci or staphylococci.

The most important item in the prognosis of healing seems to be that of nutrition, an item which unfortunately cannot well be tabulated. After a pretty careful observa-

tion of a good many cases and after much study of records this varying nutrition remains in the writer's mind the essential reason for the widely different course of the disease after operation in apparently similar cases. Here and there cases heal within three or four weeks without trouble—they will be found to be children on whom the preceding pneumonia has made but little impression. Other patients (in some of whom prompt lung expansion at the time of operation shows no "tying-down" of the lung) hang at about the same point for week after week. Some of them finally die; most of them eventually heal, but only after months. The infection is the same in these as in the other cases, the surgical condition the same, with no hindrance to lung-expansion, and yet they do not heal. If we apply suction and expand the lung, as we can do,—still they do not heal. On careful inspection of these cases we often find that even the external wound shows no formation of granulations—evidently there is a failure to form plastic material. It does not much matter in these cases what we do surgically, beyond providing proper drainage: so soon as the general condition so far improves as to provide for healing, they will, with favoring conditions, heal up; until then they will not heal. This is the explanation of the uselessness of artificial expansion in many fresh cases, and in the writer's opinion it is the question of nutrition that dominates the whole question of prognosis.

In regard to deformity following healing it may be said that temporary deformity, a flattening with some spinal curve, is the rule. In the cases seen at this stage the deformity has always promptly yielded to proper exercises. In regard to later results, especially in cases where exercises have not been carried out, the following figures are submitted:

Total 55 giving definite data.

Thirty-one without deformity enough to note.

Nine with slight difference in chest girth or with flattening, without lateral curve.

Six showed slight lateral curvature.

In two the curve is noted without note of degree.

Ten showed severe curvature. One of these was healed at 3½ months, one never operated beyond tapping, in one without data as to length of time the sinus was open. Of the others, four closed only after "over a year," 21 months, 11 months and 4½ years respectively, and three are still open.

The records show that it is possible for cases to have a long continuing sinus (in one case 9 months), and yet heal without deformity, but it is equally clear that the long continuance of a sinus predisposes to collapse of the chest wall and to consequent scoliosis, and that most of the cases not closed at 1 to 1½ years will show permanent deformity.

The *general conclusions* drawn by the writer from the data in hand are:

Empyema in children usually follows lobar pneumonia—after a varying interval.

The infection is usually with the pneumococcus.

Spontaneous cure, even when aided by tapping, is rare.

Operation should not be delayed, as time lost is strength lost, and the issue is largely one of nutrition.

The best form of operation is in general the subperiosteal resection of an inch of the eighth or ninth rib in the posterior axillary line, the evacuation of pus and fibrin masses, and tube drainage.

Irrigation at or after operation is not usually advisable.

The routine after-treatment in fresh cases should be tube-drainage, the tube being progressively shortened, and removed when the cavity is nearly healed.

Where failure to heal seems to depend on failure of the lung to re-expand, treatment by valve or suction apparatus is indicated. This is especially of value in the more chronic cases.

The mortality is about one in seven; in small children much more than in those over five. The causes of mortality are in the main beyond our control.

The great majority of cases heal even when the healing is delayed for many months. Chronic empyema, in the strict sense, is rare in children.

The closure of the cavity depends mainly on nutrition and on adequate drainage.

Recurrences may occur from faulty drainage at any time, and they may occur years after apparently sound healing without obvious cause.

Deformity of the chest is usually temporary and yields to treatment.

Long continued discharge from the cavity is not infrequently followed by chest deformity and scoliosis of a severer type, permanent and sometimes extreme.

ARTICLES V.–X.

VACCINATION AND SMALL-POX.

THE PREPARATION OF ANIMAL VACCINE.

By THEOBALD SMITH, M.D.
OF WEST ROXBURY.

VACCINATION: THE TECHNIQUE.

By EUGENE A. DARLING, M.D.
OF CAMBRIDGE.

VACCINATION: ACCIDENTS AND UNTOWARD EFFECTS.

By JOHN H. McCOLLOM, M.D.
OF BOSTON.

SMALL-POX: ITS DIAGNOSIS.

By JOHN T. BULLARD, M.D.
OF NEW BEDFORD.

LEGISLATION WITH REFERENCE TO SMALL-POX AND VACCINATION.

By SAMUEL W. ABBOTT, M.D.
OF NEWTON CENTRE.

SMALL-POX: ITS MEDICAL TREATMENT.

By MICHAEL KELLY, M.D.
OF FALL RIVER.

READ JUNE 10, 1902.

ARTICLE V.

THE PREPARATION OF ANIMAL VACCINE.

By THEOBALD SMITH, M.D.,
OF WEST ROXBURY.

THE necessity for the vaccinal protection of the individual against such a subtle and pervasive virus as that of small-pox is answered in but one way by the experience of the century just passed. Without the specific protection given the community by vaccination, small-pox cannot be efficiently controlled and suppressed.

The acceptance of this proposition by the medical profession and the State creates the responsibility of supplying as pure and efficient vaccine virus as can be made under present conditions. The preparation of animal vaccine is in itself simple enough, and this very simplicity is a source of danger, because the production may be undertaken by such as are not sufficiently trained in scientific methods to apprehend and utilize the improvements wrought by investigation, as well as to anticipate and guard against the dangers due to altering conditions.* Simplicity of manufacture and the lack of a definite scientific code of proce-

* In this regard it differs from the preparation of diphtheria antitoxin, because the latter substance cannot be prepared excepting by those having had considerable technical training. Its successful manufacture necessarily implies that degree of care which also ensures the safety of the product.

dure as regards details of preparation make safeguards of all kinds all the more necessary. It is the safeguards which represent by no means insignificant items of cost, for they mean the employment of trained persons to apply such routine tests as may be deemed necessary to eliminate dangers, and they demand the free use of animals for control observations and tests.

THE NATURE OF VACCINE.

Much has been said and written upon the original source of vaccine virus as an important factor in its efficiency. Since the day of Jenner, who considered cow-pox as a derivative of grease of the hock in horses, there has been more or less fluctuation of opinion as to the nature of cow-pox itself. The findings of the celebrated Lyons Commission in 1865 controlled for a long time the views of the medical profession. This commission, while admitting that small-pox virus may be successfully inoculated into the cow, denied that it underwent any mitigation or modification, and that, returned to man, it would produce genuine small-pox. Since that time a considerable number of investigators have succeeded in transmitting small-pox to the cow and producing genuine cow-pox, and it is now quite generally regarded as a derivative of small-pox.[1] The frequent appearances of cow-pox in the early years of the nineteenth century were in all probability due to the accidental inoculation of the cow with small-pox or with vaccine from the hands of the milker, which in its turn was at some earlier time derived from small-pox by passage through the cow. Vaccine, derived from variola, the so-called variola-vaccine, was prepared by Voigt, of Hamburg, in 1881, and it has been used by him with success ever since. Recently a new stock of variola-vaccine has been prepared successfully in Munich, Bavaria.[2]

With the fact before us that vaccinia is small-pox pro-

foundly modified by its passage through the cow, and that it may be reproduced whenever needed, the assumed danger of cow-pox virus dying out may be regarded as non-existant. But the subtle differences in vaccine from different sources and the opinions of experienced men, that vaccine may in time become less efficient, makes the constant control of vaccine upon susceptible children to determine its taking qualities a matter of great importance.

THE PREPARATION OF ANIMAL VACCINE.

The microörganisms of variola and vaccinia have not yet been recognized by the microscope, and their presence can be determined only by their effects. This gap in our knowledge is largely responsible for the divergencies in the preparation of vaccine witnessed to-day. The microörganism of vaccinia is present in the proliferated epithelium of the inoculated skin of the cow, as well as in the serum or lymph which exudes from the vesicle and its base after the overlying scab or crust has been removed.* Two methods are based on these facts. In the one, now used throughout the continent of Europe, in England, and to a large extent in our own country, the proliferated epithelium along the line of inoculation is scraped away and it becomes the basis of the vaccine. It is the so-called pulp. The other method utilizes the fluid lymph or serum of the vesicle. This method is still used to a restricted degree in our own country.

In the preservation of the virus two methods are in vogue. In Europe, and to a considerable extent in our own country, the pulp is mixed with diluted glycerine and stored in sealed receptacles. Less used to-day is the

*The microörganism is also present in the internal organs of the inoculated calf, for typical vaccine eruptions were produced by the recent German Commission[3] with the fluid from crushed spleen tissue and from lymph nodes. Positive results were obtained from one calf forty-four days after inoculation. These results need repetition and confirmation.

method once universal in our country, of drying the vaccine on the ends of flattened slips of ivory or bone, the so-called vaccine points.

Let us now look hastily at the concrete processes, and in so doing let us glance first at the procedure now in general use — the preparation of glycerized vaccine.

Calves are chiefly employed. The preferred areas for inoculation are the abdomen, the inner aspect of the thighs, the scrotum, or simply one side of the animal. The thoroughly-inspected, cleansed animal is fastened upon a suitable table, and the areas to be inoculated are shaved and washed. Disinfection of the skin with alcohol, lysol and other well-known disinfectants, is variously practised and omitted by different authorities. In any case certain disinfectants must be removed with sterile water before the vaccine is applied. On this prepared area shallow incisions, barely deep enough to become reddened, are made in parallel lines about one-half inch apart. The vaccine or seed is thoroughly rubbed into the scarified area and the calf released. Much time and ingenuity has been sacrificed by German vaccine producers in the effort to obtain a proper aseptic protective dressing for the vaccinated area, but nothing has been brought forward which has received general approval, and I think that to-day most vaccine producers use no dressing, but depend on general cleanliness.

After four to six days, according to temperature and individual variation, the calf is again placed upon the table. The vaccinated surface is thoroughly washed with sterile water, and any scabs or crusts removed. A curette is then used to scrape away the slightly elevated ridges of epidermis and subjacent layers which represent the confluent eruption following the linear inoculation. This scraped material, or pulp, is thoroughly ground and comminuted in a mortar or a special machine while being mixed with a

definite amount of about 66% of glycerine. When the process of grinding and mixing with glycerine has been completed, the vaccine is, as a rule, stored for at least a month before it is sold or dispensed free, as is done, for instance, in the German Empire. The usual dilution of the pulp with diluted glycerine is one part of pulp to three or four parts of diluted glycerine. The German Commission has shown that a dilution of 1 to 10 is equally efficient and that a dilution of 1 to 20 is still satisfactory in primary vaccination.[4]

From the outline here given there is more or less departure in different institutes. During a visit to a number of vaccine plants of the German Empire and Belgium, some years ago, I was struck with the variation in minor details presumably grounded upon the experience of the individual directors, all of whom had materially contributed to the advancement of the subject through laborious investigations. In some cases older animals are preferred, but, inasmuch as calves are more easily managed, since they have a more delicate skin, and especially since they can be fed on milk, or milk and eggs, in place of the dusty hay, they are generally used. Variations were also noticed in the time allowed for the development of the eruption, in the method of removal of the pulp, in the extent of the scarified area and in the amount of glycerine added.*

The use of fluid lymph dried upon ivory points has had and still has the favor of many practitioners in our country, because of the relative simplicity of the manipulation. After scraping or curetting for the preparation of the pulp, the serum which oozes from the base of the eruption may be used to charge the points. The usual way, however, is to wait longer, up to the seventh day after vaccination. The animal is then secured, the crusts covering the scarified area

* For variations in the method of preparation in American plants see Dr. Pitfield's exhaustive paper in Public Health, July, 1896, p. 136.

are removed, and the subjacent layer of cell débris wiped away. The lymph then oozes out, or its appearance may be hastened by compressing the base of the eruption. The fluid thus obtained is transferred to points in different ways and the latter allowed to dry.

It is of considerable interest to note that vaccine dispensed by different institutes is not derived from one and the same stock. There are three sources available:

1. The virus descended from spontaneous cow-pox and continued through an indefinite series of animals—the true animal vaccine.

2. Virus obtained from animals which have been inoculated with lymph from human vaccine pustules, either directly, or indirectly through a series of calves. This is known as retro-vaccine.

3. Vaccine obtained by passing small-pox virus through the cow—the so-called variola-vaccine.

It would probably be impossible for many vaccine plants to trace the genealogy of their current stock back to its beginnings. In Germany the use of lymph from the arms of children (retro-vaccine) has been a favorite mode of rejuvenating impaired animal virus.

There has been a more or less heated discussion on the part of manufacturers in this country since the introduction of glycerized pulp, as to the relative efficiency and purity of the latter, and the fluid lymph dried on points, which it is slowly displacing. The controversy seems to me largely unnecessary. Both pulp and lymph are fundamentally the same, for both contain the microbe of vaccinia. Schulz of Berlin found that the clear fluid exuding from the human vaccine vesicle, when diluted in 2000 parts of water, still yielded excellent results when used fresh.[4] Similarly the ordinary vaccine from calves produced vesicles in a dilution of 1 to 400. There are, however, several points of difference between the dried lymph and the glycerized pulp to which I wish to allude very briefly.

In the first place, it is highly probable that the vaccinal pulp is much richer in the specific virus than is the serum or lymph. This may not be of any significance at first, but with age the diluter virus, even in a menstruum of glycerine, becomes sooner inactive than a more concentrated virus. In the second place, the virus is less tenacious of life when dried than in glycerine. Its life is furthermore of uncertain duration. Bacteriologists know that the resistance of most bacteria to destruction, when in a dried condition, depends on a variety of factors, and what is true for bacteria is probably true for the microbes of vaccinia as well. Bacteria well covered by a dry film of albuminous matter, or embedded in dried mucus, live very much longer than those not so guarded. The resistance depends furthermore on the relative humidity to which they are exposed while in a dried state. These conditions necessarily fluctuate more or less in the case of points, and make their life period uncertain. Again, the elimination of extraneous bacteria does not proceed in the same way in the dry and the glycerized virus. In the former, the evidence points to a more or less parallel destruction of both vaccine virus and bacteria. In the latter, the vaccine virus possesses much greater resistance to the glycerine than the sporeless bacteria.

These facts in general speak in favor of the glycerized pulp as a more certain and more abundant source of vaccine.* But efficiency is not the only desideratum. Purity

* Information recently received from four large private vaccine establishments in the United States, in answer to some questions addressed to them, shows that only one of the four adheres to the use of the fluid lymph and condemns the use of the glycerized pulp. The others regularly use the pulp which is (*a*) mixed with glycerine and preserved in sealed tubes or other closed receptacles, (*b*) used to charge points without the addition of glycerine, and (*c*) used to charge points after glycerine has been added. The firm preparing the clear lymph dispenses it in the form of dried points and in tubes after glycerine has been added.

I am, furthermore, informed that the demand for the fluid vaccine has been much greater than for the dry points.

and safety are of still greater importance. This brings us to a consideration of the

BACTERIOLOGY OF VACCINE.

For much of the work done on this subject we are indebted to Europe, especially to the German Commission already referred to.[3] Since only the glycerized pulp is used on the Continent, our information is more extensive and detailed in regard to it. It has been known for some years that the fresh vaccine contains large numbers of bacteria. Up to 700,000 per cc. have been found. When 50 to 70 % glycerine is added, these bacteria are slowly destroyed. After one month the number is greatly reduced. After two or three months the vaccine may be regarded as sterile. In the meantime the vaccine virus remains intact up to six months after preparation, and even longer. The bacteriology of the fluid lymph used for points has not been thoroughly studied, at least such studies have not seen the light to any extent. Though one might suppose that this fluid, presumably poorer in vaccine microbes, would also contain fewer bacteria, the recent report of Rosenau[5] does not support such inference. Of 41 points examined, an average of 4,807 bacteria per point was found. Of 51 tubes of glycerized virus an average of 2,865 bacteria per tube was found. His statement that the glycerized virus was too fresh when put upon the market seems warranted by his results, for a sufficient percentage of glycerine acting for four to six weeks should have reduced the number of bacteria below this level. The points, on the other hand, cannot be benefited by time, since the dried virus itself becomes uncertain after two months.

The micro-organisms we are now considering are largely harmless saprophytes, which are in part inhabitants of the normal skin of the calf, in part extraneous invaders of the eruption. The intimate seat of the local disease remains

free from bacteria, and none have been found in sections of the vesicles until the seventh day.[3] They permeate the superficial scab in large numbers. Among them are found fungi and yeasts, spore-bearing bacilli of various kinds, cocci generally classed as staphylococci, *B. pyocyaneus*, and, very rarely, streptococci, together with a great variety of undetermined forms. The untoward effects of vaccination, as occasionally seen in large reddened areas around the vesicle, extensive infiltration and induration of the base of the latter, together with suppuration and the formation of large ulcers, have been, as a rule, ascribed to these bacteria. About seven years ago the German Commission[3] undertook to determine what, if any, relation existed between them and the severer forms of inflammation associated with vaccination. The outcome of prolonged studies of the effect of various vaccines was, that the contained bacteria had little or no effect upon the reaction. A bacteria-free vaccine might in one case produce extensive erysipelatous inflammation, while a vaccine containing many bacteria, among them so-called staphylococci, might have no effect whatever. The examination of excised portions of erysipelatous skin failed to reveal the anticipated streptococci. The cause of these variations was the individual susceptibility or idiosyncrasy of the child, which reacted alike to vaccine, whether sterile or containing numerous bacteria.

In estimating the effect of the pyogenic bacteria of animal life upon the human subject, we must be cautious about making assumptions that they readily produce disease in man, before we have more definite proof. In the meantime, however, no pains should be spared in removing, as far as possible, all organisms whenever this can be done without impairing the vaccine virus. Such a reduction of the miscellaneous bacteria has been attempted by variously modifying the details of the process of producing vaccine.

Pads of gauze and cotton have been applied by Schulz over the scarified skin of the calf with signal success in some cases.[4] Recently Paul,[6] of the Austrian vaccine institute, has introduced, under the trade name of tegmin, a covering made up of beeswax, gum arabic and lanoline, and containing 5% zinc oxid,* which is now undergoing thorough tests in Europe. The careful application of disinfectants to the inoculated area, both before vaccination and before collecting the pulp, is growing in favor. Some have tried without much success to remove the superficial scabs or so-called sloughs, the bacteriological *corpus delicti*, before scraping away the pulp. Extensive experiments were made by the German commission to remove the bacteria by centrifugalizing and sedimenting the diluted pulp. The results were very good, but the vaccine lost so much of the specific virus at the same time that its keeping qualities became impaired. It is probable that by a combination of these improved details a bacteria-free vaccine may be obtainable in the near future, even without awaiting the bactericidal action of the glycerine.

The possible contamination of vaccine with the spores of tetanus bacilli has created consternation among those who see in vaccination one of the greatest safeguards of a dense and constantly intermingling population. The appearance of a number of cases of this dread disease in widely separated localities about three weeks after vaccination naturally enough called attention to the vaccine. But the condition is so rare—practically unknown in Germany, the most thoroughly vaccinated nation—that a second thought and a consideration of the carefully collated cases, so far as obtainable, make the suspicion seem unfounded.[7],[8]. The whole subject is so new that few data are at hand upon which to base any inference. Careful investigations will be necessary to determine to what extent, if at all, tetanus

* Actual composition not published.

spores may germinate and multiply in the vaccinal eruptions of the cow, so that the conditions may hereafter be promptly met. If it should appear that vaccine may be responsible for tetanus, only some unusual accident or gross negligence will explain its rare occurrence.[9]

We have thus far considered only the miscellaneous contaminations of vaccine. The objectionable agents due to definite diseases of the cow are few and easily eliminated. Among them, tuberculosis and diarrhœa are perhaps the only ones I need mention here. Tuberculosis is comparatively rare in calves, and it may be easily excluded by the custom now prevailing in Europe and, I believe, in our best American vaccine plants, of killing the animal after the vaccine has been collected, and making a careful post mortem examination. If disease is found, the vaccine is rejected. It has been claimed recently by some that tuberculosis is hardly worth considering as a source of danger in vaccine. It is true that the chances that tubercle bacilli may be in the vaccine are slight. It is also true that bovine tuberculosis does not appear to-day as the great danger to man which it was formerly pictured to be, but, as I stated recently in another paper, inoculation of bacteria is a process quite different from eating and inhaling them. A vaccination wound may favor tubercle bacilli which might be unable to find a foothold in the normal respiratory and digestive tracts. In short, no one will desire to use vaccine from which the suspicion of tuberculosis has not been removed when it can be done so readily. The tuberculin test, being not infallible, can not take the place of the autopsy. The latter may at the same time reveal other pathological states which might pass unnoticed in the living animal.

HOW CAN VACCINE BE SAFEGUARDED?

Vaccination being an operation prescribed by public authority, vaccine virus is placed in a position differing from

that of any other medicinal agent. The State insists upon vaccinal protection, and justly so, but under existing conditions it can do little or nothing towards assuming any responsibility for the quality of the vaccine used.

The testing or examination for purity and efficiency of animal products used in medicine is a problem of considerable difficulty. In the case of the antitoxins the task is somewhat simpler than for vaccine. There are to be considered (1) the animals yielding the product, (2) the process itself, and (3) the efficacy of the product as a protective agent. If the State should undertake to test the vaccine sold within its jurisdiction, it would be limited to such simple tests as the enumeration of the vulgar bacteria present and a microscopic examination of the vaccine itself. The animal test for stray tubercle bacilli would require four to eight weeks. The search for tetanus bacilli would be laborious and uncertain for small samples. Nothing could be inferred from such examination beyond, perhaps, the age of the virus and the original content of miscellaneous bacteria. No certain clue could be obtained as to the condition of the animals or the care used in preventing unusual and dangerous contamination. The protective power of the lymph could not be easily determined, owing to the many manufacturers and the large number of animals yielding the virus. In any case it would be impossible for the State to guarantee the purity of the many kinds of vaccine made within its border or sent in from without. The task would be enormously expensive and of uncertain value.

There are two ways out of the difficulty. One has been in vogue on the Continent for many years. Each small state or each subdivision of a large state assumes the responsibility of preparing the vaccine itself. I fully realize that in our country such public institutes, without the safeguard of absolute divorce from practical politics, without

the loyal, unselfish support of the medical profession, and without adequate financial support, would, in all probability, be a disappointment, But granting these conditions as attainable the advantages would be very great. There would be possible a close contact between the people using the vaccine and the institution preparing it. Its efficiency and safety would be subject to a continuous control at close range not obtainable otherwise.

Not less important is the advantage of publicity in improvements, as well as failures. There is a dearth of scientific publications upon vaccine in our country, which contrasts very unfavorably with the work done in Europe and the active interest manifested in the minutely detailed accounts of the vaccine institutes and the relative efficiency of their products, as announced annually by the German government. The advances made in this country in the preparation of antitoxins, for instance, have been published almost exclusively from laboratories under public authority.

According to the other alternative, the national government might appoint inspectors for the private vaccine plants, under whose supervision the virus is prepared, tested and officially stamped, just as all antitoxin sold in Germany is tested beforehand in large lots by government experts. The place where vaccine is tested most easily and economically, and where such testing would go hand in hand with a corresponding improvement of the product, is at the source — where it is made.

In conclusion, I may say that the vaccine is but one of three factors which determine the success or failure of vaccination. The others are a satisfactory inoculation technique and the proper care of the wound afterward. These important factors are to be discussed by those who are to follow me on this program.

The following are some of the more recent publications referred to in the text:—

1. Ch. Haccius. Variolo-vaccine. Contribution à l'étude des rapports qui existent entre la variole et la vaccine. Geneve, 1892.

2. Die Thätigkeit der im Deutschen Reiche errichteten staatlichen Anstalten zur Gewinnung von Thierlymphe während des Jahres 1900. Berlin, Springer.

3. Bericht über die Impfstofffrage. Berichterstatter: Dr. P. Frosch. Berlin, 1896.

4. Zweiter Bericht. Berichterstatter: Vanselow und Freyer. Vierteljahrschrift für gerichtliche Medizin u. öffentliches Sanitätswesen. 1899, 17, p. 93.

5. M. J. Rosenau. Dry points versus glycerinated virus from a bacteriologic standpoint. American Medicine, 1902, 3, p. 637.

6. G. Paul. Ueber einige Fortschritte in der Gewinnung des tierischen Impfstoffes und der Aseptic der Schutzpockenimpfung. Wiener med. Presse, 1898, p. 129 et sqq.

7. R. N. Willson. An analysis of fifty-two cases of tetanus following vaccinia. Journ. Amer. Med. Assoc., 1902, 38, p. 1147, 1222.

8. Joseph McFarland. Tetanus and vaccination. An analysis of ninety-five cases of this rare complication. Journ. Med. Research, 1902, 7, p. 474.

9. Huddleston. Medical Record, March 8, 1902.

ARTICLE VI.

VACCINATION: THE TECHNIQUE.

By EUGENE A. DARLING, M.D.
OF CAMBRIDGE.

The two considerations which govern the technique of Vaccination are that the operation shall be successful and that it shall be safe. Danger may come from the use of impure lymph, from unsuitable conditions in the patient, from infection at the time of the operation or during the development of the vesicle. A description of the technique therefore divides itself into four parts, as follows: 1. The selection of the lymph. 2. The condition of the patient. 3. The operation. 4. The after treatment.

1. The selection of the lymph.

This is perhaps the most important factor of all as regards both success and safety, and yet it is the one condition which is beyond the control of the physician. It is well recognized by bacteriologists that lymph taken directly from the vesicles on the calf contains many contaminating bacteria. When the lymph is dispensed dried on bone points a large proportion of these bacteria remain active. Probably most of them are harmless, yet the common occurrence of the pyogenic staphylococci and streptococci in lymph so dispensed constitutes a real danger. The use of

glycerine as a slow bactericide has undoubtedly much diminished, if it has not wholly abolished the danger from this source; but the glycerinated lymph has certain disadvantages which have prevented its universal adoption, and many physicians still prefer the old unpurified lymph. The disadvantages of glycerinated lymph are the slowness with which it dries, and the difficulty which some have in making it "take." The latter may be due to too great dilution, since there are considerable variations in the proportion of successful vaccinations from different lots of lymph from the same source. However, with care in the insertion of the lymph and with proper protection while drying, the percentage of successful insertions may be very satisfactory, if not the 100% claimed by some producers.

In the absence of any feasible method of supervision by the health authorities, and of any practical test of the efficiency and purity of any given lymph short of actual inoculation of a susceptible individual, there must always be an element of uncertainty as to the care exercised in its preparation. There is no reason to doubt that many of the lymphs now on the market are prepared with due precautions, but this is certainly not true of all, and the physician has no means of discrimination. The State compels the child to be vaccinated, and should furnish the lymph for the operation, guaranteed to be pure and efficient. The reasons which justify the manufacture of diphtheria antitoxin by the State apply with even greater force to the production of vaccine lymph, and it is to be hoped that the present movement to place the control of this important matter in the hands of the State Board of Health will succeed.

2. The condition of the patient.

The best age for the primary vaccination of healthy children is between three and six months, *i. e.* after the child is well started in life and before it has begun the trouble-

some process of teething. At this period the constitutional disturbance is milder, as a rule, and the care of the child is much simpler than at a later age. In the case of delicate children, or of those suffering from some constitutional disease, the vaccination may be postponed at the discretion of the physician. Vaccination is contra-indicated when the child is suffering from any inflammatory skin disease, such as erysipelas or eczema, or when it has recently been exposed to any acute infectious disease, such as scarlet fever, measles, mumps or whooping cough. In time of epidemic, or when there has been an actual exposure to small-pox, vaccination may be performed as early as the first week of life, or in the face of circumstances which would ordinarily contra-indicate it; but in individual cases the decision as to its advisability must be left to the judgment of the attending physician.

There is considerable difference of opinion as to the proper time which should elapse before revaccination. In most cases children who have been vaccinated in infancy cannot be revaccinated successfully until after a period of eight or ten years. In Germany, where the matter has been regulated with great care, the law requires vaccination in infancy and revaccination at the age of twelve. The almost complete immunity from small-pox which Germany has enjoyed since the enactment of this law is attributed largely to the compulsory revaccination, and it is reasonable, therefore, to recommend this practice. After two successful vaccinations it is not necessary to repeat the operation except in times of epidemic, when universal revaccination should be insisted upon. The plan which has been suggested of repeating the vaccination at stated intervals, until there is complete insusceptibility, is theoretically correct, but is practically too drastic to be enforced, except in the case of physicians and nurses who are thrown into close contact with cases of small-pox.

3. The operation.

The physician should remember that the operation of vaccination, though insignificant in itself, demands the observance of the ordinary rules of surgical asepsis. The hands of the operator and the skin of the patient should be surgically clean, and only a clean sterile instrument should be used for the scarification. With a little care it is possible to perform the operation without touching the skin of the patient with the fingers, and this source of infection may be entirely eliminated. The most convenient place for the insertion is the outer side of the upper arm. There is no objection to vaccinating on the leg in the case of girls, where it is desirable to avoid disfigurement; but in this location the after treatment requires somewhat greater care to ensure protection and cleanliness. The skin of the patient should be washed thoroughly with soap and water, and afterwards with alcohol or ether, with enough friction to cause some reddening. This removes the superficial layer of the epidermis and dilates the cutaneous vessels, thus aiding in the absorption of the lymph. If there is any hair it is well to shave a small area, but this is rarely necessary. Antiseptics as a rule are not required. For the scarification the best instrument is an ordinary needle, which should be sterilized just before use by heating in an alcohol or Bunsen flame. The lymph may be placed on the skin first, and the scarification done through it, or the skin may first be scratched and the lymph then well rubbed and pricked in, the needle being used for this purpose. It is better to make three or four small insertions than one large one. A scarification no larger than an eighth of an inch in diameter will produce a vesicle and cicatrix from one quarter to one half an inch in diameter. When several insertions are made they should be at least an inch apart. If placed too near one another they are liable to coalesce and form a single large vesicle, which is much more likely to be injured than several small ones.

4. After care.

The arm should preferably be left bare until the lymph has dried. If glycerinated lymph has been used, this may take a considerable time — an hour or more — and in office practice this may cause much inconvenience. When this long wait is impracticable, it is permissible to use a shield for temporary protection. The best shield for this purpose is one of the light linen or paper forms, porous or perforated, and stiff enough to resist crushing. This may be held in place by two or three small strips of plaster. The shield may be left on for a few hours, and should then invariably be removed. The subsequent dressing should be the simplest compatible with cleanliness and adequate protection. It is sufficient for the first five days to wind a strip of sterilized gauze, or a clean handkerchief, about the arm and hold it in place by pinning it to the under garment. After the vesicle has formed, this same dressing is still sufficient in most cases. If there is danger of breaking the vesicle, or if the patient is inclined to scratch it, it may be well protected by a cocoon of absorbent cotton and collodion. A very comfortable dressing may be made by using two square pads of folded gauze, one three inches in diameter and the other a little smaller. The larger one has a hole cut in its centre, large enough to include the vesicles, and is held in place by two strips of zinc oxide plaster. The smaller pad is then pinned on so as to form a lid over the hole. This dressing has the advantage of permitting the inspection of the vesicles without disturbing it. If the vesicles break down and suppurate, they should be washed daily with boiled water or a mild antiseptic, and kept bandaged with sterilized gauze. If the itching is intolerable, a little aristol or some other mildly antiseptic powder may be dusted on. Occasionally a scab forms having a few drops of pus under it and some œdematous granulations. Such a scab should be carefully removed,

and the granulations should be cut off with a pair of flat-curved scissors. This proceeding is entirely painless and leaves a clean, healthy surface, which usually heals rapidly under mild antiseptic treatment.

Shields as permanent dressings are to be condemned, without exception. The transparent celluloid ones, which have been used so widely of late, are especially bad. If insufficiently ventilated they confine the perspiration, and this macerates the skin of the enclosed area, making it more or less red and œdematous. If the sides are perforated, to allow the free access of air, lint is rubbed off from the inside of the under shirt and accumulates inside the shield. Sometimes this lint forms a feathery mass looking much like a growth of mould. It is, of course, not sterile. Furthermore, the circle of plaster, which is employed to hold the shield in position, is very irritating to delicate skins, and often causes an eczematous eruption which greatly increases the discomfort of the patient. The only advantage which shields possess is their stiffness, which affords more protection against injury to the vesicles than a simple bandage, but this advantage is wholly counterbalanced by the disadvantages mentioned.

The simpler the after treatment, the more likely it is to be carried out. The physician should always give detailed instructions to the patient, or, if the latter is a child, to the parents or nurse. This precaution is often neglected by the busy practitioner, or the instructions given are so general as to be of little value. Emphasis should be placed on the importance of cleanliness and protection from injury. The patient should be warned not to scratch the arm nor to prick the vesicles. He should also be cautioned not to apply vaseline or any other greasy substance to relieve the itching, as this softens the epidermis and may cause the vesicles to break down and suppurate. Ignorance and carelessness on the part of some patients are unavoidable,

but personal attention to details on the part of the physician will do much to obviate this source of danger. The responsibility resting upon the physician in this matter is not a light one. If mishaps occur he is always blamed, whether justly or not. The sources of danger, as already pointed out, are not entirely under his control, but it is his duty, as well as his interest, to observe every precaution which will minimize the danger of vaccination, and yet afford his patients that immunity from small-pox which is one of the greatest blessings conferred on mankind by the art of medicine.

DISCUSSION.

Dr. J. H. McCollom, of Boston: I heartily coincide with Dr. Darling's ideas, and it seems to me that he has brought up some very interesting points.

Having had a large experience, both before and after the epidemic of last fall and winter, I must confess that I think the majority of sore arms are due not so much to the carelessness of the individual, as to the physician in not giving sufficient directions.

ARTICLE VII.

VACCINATION: ACCIDENTS AND UNTOWARD EFFECTS.

By JOHN H. McCOLLOM, M.D.
OF BOSTON.

In this paper no attempt will be made to prove the protective power of vaccination, a self-evident proposition, neither will anything be said regarding the technique of vaccination; these subjects having been ably treated in other papers presented to the Society to-day. It must be taken for granted that vaccination is an operation and that any operation no matter how slight may be followed by injurious effects, if there is carelessness in its performance or in the subsequent care of the wound. When we take into account the immense number of individuals vaccinated each year and the remarkable rarity of any serious results, the wonder is, if vaccination is such an injurious procedure as is claimed by its opponents, that there are not more disastrous effects. Many of the alleged injurious effects after careful investigation have been proved to have no connection with vaccination either directly or indirectly, but have been shown to be due to carelessness on the part of the individual.

It has been alleged that syphilis is frequently caused by vaccination. While this may have been possible in certain rare instances when humanized lymph was used, it is impossible when bovine lymph is employed. It has happened

to the writer in several instances to investigate alleged occurrences of communication of syphilis, but in each case careful investigation revealed the fact that the individuals were suffering from the primary lesion of syphilis at the time they were vaccinated and that vaccination was used as a cloak to cover this condition. As an illustration of the manner in which vaccination is unjustly accused of conveying syphilis Dr. Cory, in his Theory and Practice of Vaccination describes very aptly the following case of a child whose father contracted syphilis shortly after his marriage. Dr. Cory says: * "It was the second child of the marriage; the first died within a very short period of birth. This child, being apparently healthy, was vaccinated at St. Thomas's Hospital in 1878. It was duly inspected, and was brought again with the eruption, which first appeared on the tenth day after vaccination. This is a very good example of the cases of alleged syphilis after vaccination. It is so tempting for parents to lay the results of their own iniquities upon vaccination, especially when they are encouraged so to do by shameless agitators. To enable us to view the truth of these allegations against vaccination, we will suppose a man to have had syphilis and to play a game of football; he receives a kick on the shin, and in due course a syphilitic node may develop from the bruise. The man would be as entitled to say he acquired his syphilis from playing football as a parent of a child who has had syphilis would be to say that the child acquired its syphilis from vaccination."

Erysipelas may follow vaccination but there is no proof that the specific organism of this disease is in the vaccine lymph. Erysipelas also frequently follows the scratch of a pin or any slight abrasion, but this is no argument against vaccination but it is an argument for the careful protection of the vaccine vesicle. Urticaria may follow vaccination

* Lectures on the Theory and Practice of Vaccination. By Robert Cory, M.A., M.D. Cantab., F.R.C.P. London. Page 65.

just the same as that the ingestion of fish and other food may also cause this troublesome eruption. Impetigo contagiosa sometimes appears after vaccination, but as impetigo is an extremely common disease there is no proof that the vaccination is the cause of this manifestation, as the disease is fully as frequent among children who have not been recently vaccinated as among those who have been recently vaccinated.

Abscesses in the axilla may follow a vaccination but this condition is extremely rare and in my experience it has always been due to some carelessness on the part of the individual.

The site of the vaccination may slough, but I have never seen any serious result other than a somewhat tedious healing follow this condition. When this sloughing takes place it is always due either to a diminished vitality of the individual or carelessness in using the arm.

It is doubtful if cutaneous tuberculosis is ever communicable by vaccination, but the abraded surface if brought in contact with the sputum of a tuberculous patient may be infected. This is only another argument in favor of carefully protecting the site of the vaccination. As illustrating how the abraded surface may become infected with micro-organisms the history of the following case is of interest. A child four years old was vaccinated with bovine lymph in the usual way. The vaccine vesicle passed through its regular stages and in due process of time the crust separated leaving a slightly abraded surface. Shortly after this a small patch of a membrane having the clinical appearance of a diphtheric membrane appeared on the abraded surface. Cultures taken from this membrane revealed the presence of the bacilli of diphtheria. The child was not seriously ill and after a short time the membrane disappeared. Investigation proved that a sister of this child was suffering from a mild attack of nasal diphtheria, as proved by a bacteriological

examination, and no doubt infected the vaccinated patient with the micro-organism of diphtheria. Certainly vaccination of itself could not have caused diphtheria. If diphtheria can be communicated in this way certainly tuberculosis may be.

Within the past year there have been several instances in the country of tetanus alleged to have been communicated by vaccination. It is a matter of very great doubt if the vaccine lymph, unless prepared with a total lack of care such as is incomprehensible, ever contains the bacillus of tetanus. Dr. Joseph McFarland has investigated this subject very carefully and throughout the whole extent of the country he was unable to collect more than 95 cases. Considering the immense number of people vaccinated during the past year, if the lymph was at fault, it is hard to understand why there were not more cases of tetanus. An equal number of injuries with a toy pistol or a rusty nail would be followed by a vastly greater number of cases of tetanus. The conclusion which Dr. McFarland reached in his exhaustive paper are as follows:*

"1. Tetanus is not a frequent complication of vaccination, a total of ninety-five cases having been collected.

2. The number of cases recently observed is out of all proportion to what has been observed heretofore.

3. The cases are chiefly American and occur scattered throughout the eastern United States and Canada.

4. They have nothing to do with atmospheric, telluric, or seasonal conditions.

5. They occur in small numbers after the use of various viruses.

6. An overwhelming proportion has occurred after the use of a particular virus.

7. The tetanus organism may be present in the virus in small numbers, being derived from the manure and hay.

* The Journal of Medical Research. Vol. VII., No. 4. May, 1902. Page 492.

8. Occasionally the number of bacilli becomes greater than usual through carelessness or accident.

9. The future avoidance of the complication is to be sought for in greater care in the preparation of the vaccine virus."

During the past year there have been eighteen instances of dermatitis herpetiformis in Boston and vicinity following vaccination, and it has been alleged that this disease was due to vaccination. While it is perfectly true that these attacks followed vaccination there is no proof that vaccination caused them, as this disease has been observed in many cases where the individuals had not been vaccinated. Attacks of this disease occur after confinement and are described in works on dermatology. One instance of this kind has come under my notice. Several other cases which could have no possible connection with vaccination have also been observed. I must confess that I have been unable to gain any light on the etiology of this disease. The following account of the autopsy on the body of a man who died from this disease, taken from the records of the Pathological Department, Boston City Hospital, where the autopsy was made, may be of interest. It must be borne in mind that although the man had been vaccinated some five or six weeks previous to his illness there was no irritation at the point of inoculation and nothing to indicate that the vaccination had anything to do with the disease.

* CLINICAL DIAGNOSIS, DERMATITIS HERPETIFORMIS.

Autopsy 13 hrs. post mortem.

Age 42 yrs. Body length 174 cm.

White man, strongly built, fairly nourished. Body warm. Some rigor; lividity of dependent parts; pupils normal and of equal size; no œdema.

Scalp covered with scaly crusts. Corners of mouth and nose thick with dark clotted blood. Exudate on lids, and

* Taken from Records of Pathological Department, Boston City Hospital.

left lid discolored. Over clavicles a large excoriated dark red area on either side, irregular, not deep, showing thin crusts at borders and over part of its surface. The whole back from neck to fold of buttocks a mass of similar dried lesions. These areas also are distributed over chest and in axillæ and groins. Surface of the palms show scaly crusts. Nowhere are there vesicles or blebs, all the lesions being dry. Lower legs free except lesion size of a quarter over head of left fibula and one over left outer surface of leg above malleolus. Deepest excoriations appear to be over coccyx, centre of back, and right buttock. Elbows, outer side of right and inner aspect of left running up to above deltoid tubercle show deep involvement with lesions similar to those described above.

Peritoneal cavity.—Appendix lies behind the cæcum, is curled in a "watch spring" spiral, has no mesentery, is 6 cm. long and is adherent to the cæcum. Mesenteric lymph nodes are normal. Diaphragm extends to 5th space on the left side, 5th rib on right side. There is nothing notable on the peritoneum covering the intestines, stomach or parietes.

Pleural cavity.—On removing the sterno-costal trap the lungs bulge forward prominently, nearly meeting in the median line and partially obscuring the pericardium. The lungs are everywhere free save for a few fibrinous shreds, stretching from the left lower base of the underlying diaphragmatic pleura. The left pleural cavity contains about 30 cc. straw-colored clear fluid.

Pericardial cavity.—The pericardium is everywhere normal. There are about 25 cc. of straw-colored clear fluid in the cavity. Pulmonary artery opened in situ contains white elastic non-adherent clot.

Heart.—Normal size. A trifle flabby. Cut surface of myorcardium is brownish red. Endocardium presents the tiger-lily picture. With the exception of the mitral, the valves are normal. The mitral segments are retracted, and thickened irregularly at the borders; the chordæ tendineæ are shortened and stiffened. Coronaries normal.

Frozen section of the myocardium shows minute fat droplets in the intrafibrillar sarcoplasm.

MEASUREMENTS:	Tricuspid valve,	12.5	cm.
	Pulmonary valve,	7	cm.
	Mitral valve,	9.5	cm.
	Aortic valve,	7	cm.
	Left ventricle,	1.5–2	cm.
	Right ventricle,	5	cm.

Lungs.—Pleura smooth, dirty gray in color with usual carbon tracery. Right lung voluminous; on section marbled by alternating areas of bright red and gray; the great part crepitant, but throughout are firm airless areas somewhat granular. In places fine bubbles of air or dirty puriform fluid exudes on pressure.

Left lung.—Upper lobe similar to right lung except for fewer areas of hyperæmia and more œdema. Lower lobe solid, airless. The diaphragmatic surface of this lobe is roughened by yellowish fibrinous tags. Section at this point leads into numerous small cavities near the surface filled with extremely foul-smelling greenish necrotic material, smear from which shows numerous lanceolate cocci in pairs and short chains, smaller flattened cocci in chains and a stout bacillus all staining by Gram. The remainder of the lobe is dirty reddish gray in color, firm, moist, with a certain tenacious, almost mucoid exudation on the cut surface admixed with numerous air bubbles. A section sinks in water.

Beginning with the primary bronchus, the bronchi everywhere throughout both lungs are filled with a dirty gray, at times blood-tinged, offensive muco-purulent exudation. The bronchial lymph nodes are deeply pigmented and present nothing remarkable on section.

Spleen wt. 120 gms. Capsule wrinkled. Pulp soft, red-brown. Malpighian bodies appear as irregular spheroids of gray white, measuring sometimes 2 mm. in diameter. Trabeculæ and blood-vessels prominent.

Liver wt. 2320 gms. Surface smooth, yellowish red. Cut section yellow-red with occasional pale area. Markings fairly distinct.

Pancreas normal.

Kidneys wt. 390 gms. Capsule peels readily, leaving smooth surface. On section dark brownish red, markings regular, glomeruli evident. Cortex measures 7 mm. to 1 cm. Frozen section does not show fat.

Adrenals rather large; normal in appearance on section.

Bladder not remarkable. Prostate of average size; testicles pull out readily.

Aorta normal.

Organs of neck.—Pharynx rather injected and granular, but no sign of vesicles or ulcerations either about tonsils, uvula or tongue.

The larynx and trachea are covered in irregular patches by a yellowish green muco-purulent material which in one place forms a narrow line extending from the larynx to the bifurcation of the bronchi. This substance is extremely tenacious, being removed with difficulty from the tracheal mucous membrane. Besides this the trachea contains a considerable amount of dirty gray semi-fiuid material, which empties itself on section of the organ.

Gastro-intestinal tract.—The lesser curvature of the stomach shows a patch 6 cm. in diameter, composed of discrete and confluent tuft-like areas of capillary injection. A few small hæmorrhagic spots observed elsewhere on mucosa.

Save slight injection of surface of rugæ at irregular intervals, the small intestine is normal. Colon shows slight injection in its upper part.

Brain wt. 1485 gms. Normal. Pial surface quite wet.

ANATOMICAL DIAGNOSES.

Extensive lesions of skin.
Gangrene of lung (left lower lobe).
Acute lobar pneumonia (left lower lobe).
Broncho-pneumonia (right lung, left upper lobe).
Acute bronchitis, tracheitis, laryngitis and pharyngitis.
Acute gastritis.
Hydrothorax.
Chronic mitral endocarditis.
Fatty degeneration of heart.
Acute splenitis.
Acute fibrinous pleuritis (left lobe).

While the result of this autopsy explains the cause of death, it does not throw any light on the etiology of the disease. So far as is known the bovine species are not af-

fected with any disease resembling in the slightest degree dermatitis herpetiformis. If vaccination was the cause of the cutaneous manifestation in this instance, it would be reasonable to suppose, considering the immense number of people who have been vaccinated in the past hundred years, that some physician would have recognized and reported similar cases. The literature of vaccination is sterile on this point.

In the Sixth Report of the Royal Commission of England, Appointed to inquire into the Subject of Vaccination with Minutes of Evidence, and Appendices, the following statement is found: —

* "It is true that in a considerable number of the cases examined for us the injury or death is reported to have been only indirectly due to vaccination. Insanitary surroundings and parental ignorance, or even parental neglect, are assigned in some cases as the causes of complications. But even in such cases it is clear that, apart from the vaccination, the contributory causes alone would not have produced the results admitted. An operation which for its safety requires complete sanitation, with care and skill on the part of every mother, would seem to be scarcely a fit matter for universal compulsion."

This paragraph was written by the opponents of vaccination after a most searching investigation, taking many months, and they were unable to bring forward a single instance of death due directly to vaccination. There is no mention of any case of dermatitis herpetiformis. If the cases to which allusion has just been made were the direct result of vaccination, the question very naturally arises, why have not these cases been observed in England as well as in this country?

In order to obtain the consensus of opinion of the mem-

* Sixth Report of the Royal Commission at England, appointed to inquire into the Subject of Vaccination, with Minutes of Evidence and Appendiees. 6 & Final. 1896-1897.

bers of The Massachusetts Medical Society, the following circular was addressed to each member of the Society:

BOSTON, April 15, 1902.

DEAR DOCTOR:

At the next meeting of The Massachusetts Medical Society one of the subjects for discussion will be Vaccination. I have been asked to collect statistics regarding its injurious effects. You will confer a favor on the Society by answering the following questions.

Please mail as soon as convenient, in the enclosed envelope, your replies.

Yours truly,

JOHN H. McCOLLOM.

TO DR. ——

1. How long have you been in practice?
2. How many individuals, approximately, have you vaccinated?
3. What injurious effects, due to vaccination, have you observed?

Twenty-seven hundred circulars were mailed and replies have been received from eighteen hundred and fifty-nine individuals. It was thought that in this way valuable information from a large number of physicians practising in different parts of the country could be obtained. The experience of 1859 physicians whose professional life ranged from six months to sixty years, aggregating 29,631 years of practice and 2,525,083 vaccinations, would be valuable testimony for or against vaccination, so far as injurious effects are concerned.

In the replies from 1701 physicians the statement is made that no injurious effect has followed vaccination. Moderately severe effects, in which the individuals were unable to attend to their business for four or five weeks, were reported in 68 instances. In 54 instances there were extremely severe results of vaccination; namely, severe cellulitis, 28; erysipelas, 9; extensive sloughs, 5; poisoning from the dye of underclothing, 2; cases of dermatitis herpetiformis, 6. In two instances where diabetic patients were vaccinated there

were somewhat extensive sloughs on the arm, which eventually healed. There were two cases of syphilis alleged to have been contracted from vaccination. The deaths reported to have followed vaccination were 35; namely, tetanus, 11; erysipelas, 4; dermatitis herpetiformis, 12; septicæmia, 3. In two instances the cause of death was not given. In one case tuberculosis was alleged to be due to vaccination. In one instance a chronic nephritis was said to be exacerbated sufficiently by vaccination to cause the death of the individual, and one death came from pyæmia. In regard to the deaths in six instances, the connection between the vaccination and the death of the patient was certainly problematical. Of the 11 deaths from tetanus two were certainly not connected with vaccination. In one of these cases, although tetanic convulsions were present, the autopsy proved the existence of tubercular meningitis. In the other there was a septic wound which it is reasonable to suppose may have become infected with the tetanus bacillus.

As illustrating the protective power of vaccination the following statement of a physician, who was a medical student in 1857, is of interest. This man, after having been vaccinated with humanized lymph, inoculated himself two or three times with virus taken from a patient ill with the small-pox, without any injurious results.

One physician who was a surgeon in the army during the Spanish-American war reports that the 8th Massachusetts Regiment was revaccinated four times during a year, and also that the 46th Regiment was revaccinated seven times without causing loss of time on account of sore arms. A Health Officer in one of the cities of a neighboring State, reports that he vaccinated 30,000 individuals without any deleterious results.

The report from an institution where 1500 prisoners were vaccinated without injury would seem to prove the innoxiousness of the operation.

In the United States navy a surgeon in the service reports 5,000 vaccinations without a single man being placed on the sick list. The reports from many of the smaller institutions also show a similar immunity from particularly sore arms.

During the past year a very large proportion of the attendants and patients at the Boston City Hospital have been vaccinated. I have carefully watched these vaccinations and I have failed to discover any instance of marked discomfort to the individuals.

At the South Department during the year vaccination has been general. In no instance have the patients or attendants suffered anything more than a slight and temporary discomfort. It is certainly true that vaccination is unjustly held responsible for many manifestations which would occur in the natural course of events if the individuals had not been vaccinated. For instance, one physician reports a case of appendicitis occurring one week after the patient was vaccinated, and the same man also states that a patient who had been vaccinated four days previously developed an abscess of the face due to a carious tooth. It is due to this man to say that he did not attribute these occurrences to the vaccinations, but that the relatives and friends of the patients did.

Two instances of death following vaccination, occurring in a neighboring city and attributed by many of the friends to vaccination, are examples of the manner in which vaccination suffers. One was the case of a child who was vaccinated four days previous to a fall which caused a fracture of the skull, the other was that of a patient who died from pneumonia one week after vaccination. I have investigated some six deaths alleged to have been due to vaccination where there was no irritation of the arm, and the cause of death was, without the shadow of a doubt, due to gastro-enteritis.

If the reports of the 2,525,083 vaccinations are summarized it will be found that in 2,516,684 cases there were no injurious results; that in 68 instances there were moderately severe results; that in 54 cases extremely severe effects were noted, and that the deaths numbered only 35.

If 2,525,083 subcutaneous injections of morphia in ordidary doses were given there is no doubt that there would have been a much larger number of deleterious effects.

If there had been a simple scarification of 2,525,083 arms there certainly would be nearly as great a number of cases of cellulitis and erysipelas as has been found after vaccination, and there is no doubt that there would have been a certain number of deaths due to the abrasion of the cuticle.

It would seem, therefore, both theoretically and practically, judging from the reports of 1859 physicians, that the following conclusions are justifiable: —

1. That vaccination should be carefully done with aseptic precautions.
2. That the after treatment of the site of the scarification is very important in preventing cellulitis, erysipelas and tetanus.
3. That vaccination when carefully done with properly prepared lymph is a harmless procedure.

DISCUSSION.

Dr. Francis D. Donoghue, of Boston: The case I wish to report is of extreme interest from the fact that without operation a fatal case of peritonitis might have been recorded as attributable to vaccination. The case occurred in the practice of Dr. R. P. Loring of Newton Centre, who conducted the post-operative treatment, and to whom I am indebted for notes of the case, and was seen and operated upon by Dr. Mixter, who has permitted this report.

Female, aged three years eight months. Was vaccinated on the left leg, Oct. 25, 1901. The vaccination pursued the usual course for a successful take.

On Nov. 4, she was taken with pain in the abdomen indistinct in location. There was rather more tenderness in the right iliac region than elsewhere, occasional vomiting, pulse 140, temperature 104.

Nov. 5, no change. Nov. 6, seen by Dr. Worcester of Waltham, in consultation with Dr. Loring. Symptoms same as on 4th, pulse 120, temperature 103. Nov. 7, seen by Dr. Mixter. Not much change in the condition. Glands in left groin slightly enlarged and tender, but there were no localizing abdominal symptoms. On account of the indefinite nature of the symptoms no operative interference seemed indicated. Nov. 9, condition about the same, except for some rigidity of the right rectus muscle. Temp. 104, pulse 140

Longer delay did not seem advisable, and laparotomy was performed. Incision was made through right rectus muscle, and a gangrenous appendix was found in a partially walled off cavity containing pus. Culture taken showed pure colon bacillus.

Appendix removed, incision closed and gauze drainage. Convalescence uninterrupted.

Dr. G. H. FRENCH, of Dorchester: I would like to ask the writer how he would consider the question of vaccinating a person who had at the time some acute febrile disease. Would it be safe, for instance, to vaccinate a patient sick with a severe attack of the grippe?

Dr. MCCOLLOM: In answer to the question, I would say, if the patient has been exposed to small-pox, I should certainly vaccinate. I speak with some assurance, because I have vaccinated patients in the South Department with scarlet fever and diphtheria during the acute stage of these diseases, and have seen no ill effects from so doing.

One point which I would like to have brought up here is the vaccination of pregnant women. It seems to me very important.

ARTICLE VIII.

SMALL-POX: ITS DIAGNOSIS.

By JOHN T. BULLARD, M.D.
OF NEW BEDFORD.

In many cases the diagnosis of small-pox can be made at once from the eruption itself and without hesitation, while in others the decision can be made with certainty, but only after a few days of observation. It is in the milder and modified forms of small-pox, however, that considerable difficulty or even impossibility in making a correct diagnosis occurs, and it is through these patients that the disease is usually transmitted to a large number of others. It becomes often a most difficult matter for a health officer, called in perhaps for the first time in the stage of desquamation, to decide from the imperfect history he is usually able to glean, whether the case has been one of small-pox, should be immediately removed, a large number of people quarantined, or whether the malady has been a varicella and no precautions are demanded.

It should be borne in mind that several facts beside the eruption are of value, and should receive consideration in each case.

The first question that should be asked is: Has the patient in any way been exposed to small-pox? In epidemics where exposures can be easily traced, we are enabled to de-

cide very early in the disease what we have to deal with, and are justified in sending our patients early into quarantine, thereby lessening the danger to others. While the history of exposure can not, many times, be ascertained, it is well to remember that no case of small-pox originates *de novo*, and careful questioning may reveal some friend or neighbor who has had a recent eruption, and investigation of the latter case may throw considerable light upon the one under consideration.

Secondly.—Has the patient been vaccinated, and if so how recently? In an epidemic of about 175 cases in New Bedford in 1900, nearly all occurring in French-Canadians, 95% of the victims were unvaccinated, and in many households the unvaccinated were affected and those vaccinated escaped. A pertinent question also, is, "Has the patient ever had chicken-pox?" Age also has some diagnostic value. While chicken-pox is essentially a disease of childhood, small-pox attacks all ages equally.

Next come the constitutional disturbances which occur before the eruption. Notice the sudden onset, the rise in temperature, rigors or chills, often nausea and vomiting, severe frontal headache, and backache,—in fact, the usual symptoms of any acute febrile disorder. All writers lay especial stress on the severe and peculiar backache corresponding to the lower dorsal, lumbar and sacral vetebræ, and some state that it is hardly ever absent, and is almost "pathognomic."

Especial care was taken by the writer and his associates on the Health Board to investigate this symptom during the epidemic above referred to, with the result that they found it not at all a prominent symptom, being absent entirely in many cases, and in others complained of as merely a dull aching low down in the back. Severe frontal headache was much more prominent and frequent. The consensus of opinion, however, points to backache as of consider-

able diagnostic value. In mild cases these prodromal symptoms may be present either with the same intensity as in the severe ones, or they may be greatly modified and not pass beyond a period of feverishness and malaise.

If a diagnosis is attempted at this stage, influenza must be remembered. The symptoms are very similar, and many errors have been made, by both physician and patient thinking that the case was one of grippe, the patient being discharged before the appearance of the eruption.

During the stage of invasion, usually on the second day, in a small proportion of cases a so-called initial rash of two different forms may appear, either erythematous as a scarlatina or blotchy as in measles, and may lead to an error in diagnosis; although sometimes general, as a rule they affect only the lower part of the abdomen and inner surfaces of the thighs, usually are transient, fading away in twelve to twenty-four hours. Here the diagnosis may be made of scarlet fever. The longer duration and the severity of the premonitory symptoms, the unusual location of the rash, the duller red color of the erythema and the absence of severe angina and enlarged cervical glands should enable the physician to at least lean toward small-pox. Erysipelas should be borne in mind, but its method of spreading and its peculiar glazed appearance do not offer serious difficulty in recognition.

In the papular form of initial rash, we have a certain similarity to measles. The catarrhal symptoms of measles are not prominent in small-pox.

These prodromal rashes last only a few hours, and are not a serious obstacle to diagnosis.

The true eruption itself must now be considered. On the fourth day macules appear on the forehead, around the roots of the hair, and about the mouth, nose and eyes, the temperature drops to normal, and in mild cases may remain so until the end of the disease. I know of no other

disease where the temperature curve acts like this. These macules become papules and indurated, and by the next day present to the touch a shotty feeling, due to the anatomical arrangement in the skin. These lesions by the next day or two appear on the trunk and extremities, the feet being the last part affected.

Then follow the vesicles, at first minute and then larger, then umbilication, and about the sixth day of the eruption pustules, suppuration, fever, and lastly dessication, taking place first in the centre of the pustule and finally the transformation into a dark brown or blackish crust.

This eruption may be discrete or confluent. It may also be hæmorrhagic when ecchymoses appear in the skin and conjunctivæ in the prodromal stages and into the lesions themselves afterwards.

These are the so-called "text-book cases" and can be quickly passed over as the diagnosis is readily made, and we will consider the milder cases and with what diseases they may be confounded.

In so-called varioloid, called also modified small-pox and pseudo small-pox, occurring especially in the vaccinated, and in certain recent epidemics, the errors in diagnosis are frequent, and the fact that in many cases the services of a physician are not called for, does much to favor the spread of the disease. The erythematous initial eruption is mentioned as being more often met with in the milder epidemics, but in our experience it was seldom met with, or if present was of so transient a nature that it escaped observation. To add to the difficulty of diagnosis the eruption often is abortive, going no further than the papular stage; or it may progress to a small vesicle which soon shrinks; or may become sero-purulent, stop there and dessicate. We had two cases that were surely small-pox, in which after some severe constitutional symptoms the eruption never developed at all.

There are many cases where there is no secondary fever, and the patient is convalescent after the appearance of the eruption, which may be extremely scanty.

I had the opportunity to see one case recently where with moderate prodromal symptoms the rash was limited to a few lesions on the face, perhaps half a dozen, only two of which became pustular. A few papules appeared on the chest on the following day. These lesions quickly dried up, but there was considerable induration, and knowing the exposure and history of the case there was no doubt that the disease was small-pox, occurring in a man who had been successfully vaccinated some eight years previously. Here, without the history, the diagnosis would have been extremely difficult.

What eruptions are liable to be confounded with small-pox?

PUSTULAR ECZEMA may at first deceive the practitioner. The history of the case, including absence of constitutional symptoms, the absence of throat symptoms, and the distribution of the rash, should enable the physician to make a diagnosis. There is also a lack of well-defined progressiveness to the lesions. The vesicles and pustules are small and arranged on an inflamed base which is larger than the papule of small-pox.

SYPHILIS.—The similarity is often so close as to deceive even the most experienced. There is an identity of anatomical forms and also of evolution through the stages of papule, vesicle and pustule. Hutchinson says: "The simulation of the variolus eruption by syphilis is the most marked example of syphilitic imitation. The papules are elevated, shotty to the finger, have depressed centres, affect the same regions as small-pox, and resemble it so absolutely that nothing but the history of the case can help the surgeon to a correct opinion."

Added to this often occur a high fever and well-marked umbilication. To distinguish this from small-pox we must rely upon the history of the disease and the presence of other signs of syphilis and the constitutional symptoms. In most cases of syphilitic eruption there are flat, copper-colored, scaly papules along with the vesicles, and the rapid changes which occur in the small-pox lesions are not present in syphilis. The temperature as a rule does not drop with the outbreak of the eruption.

IMPETIGO.—During the recent epidemic, many cases of impetigo were reported to our local health board. In some cases the diagnosis was made with difficulty, inasmuch as many of the small-pox cases assumed a bullous form of eruption.

The constitutional symptoms of small-pox are lacking, there is but little surrounding infiltration, the lesions rising abruptly from the surface much as in the blebs of pemphigus.

In impetigo contagiosa the lesions rarely exist in numbers, have more crusting than in small-pox, and the constitutional disturbance is slight.

While ACNE should present no difficulty of diagnosis from small-pox, the writer desires to report a case where an error was made by attempting to make a snap diagnosis.

In January last, a man walked into the New Bedford Board of Health office stating that he had small-pox, and desired to be sent to the hospital. He added to this unusual statement that he had had small-pox twice before, once ten years and once six years previously. As an immune he had been nursing small-pox for the last month in a neighboring city, and left there two days before, and had come to New Bedford for employment. He gave no history of backache, fever, or headache, but said for the last few days he had not felt well and knew he had the small-pox, pointing to his face as evidence. On the face were several characteristic lesions of acne, and in addition four small papules, two upon the

forehead and two upon the cheek. These papules were not indurated. There was nothing else upon the body. There was nothing upon the mucous membrane of the throat. The temperature was 98.2. Taking into consideration the man's statement of having had small-pox on two previous occasions, the absence of constitutional symptoms and the presence of acne, the writer considering he had to do with one of those persons who whenever he has been exposed to a disease thinks he has contracted it, flatly refused the man's request to be transferred. His address was, however, taken, and two days later the man was found sick with small-pox which proved to be of average intensity. The eruption when seen must have been only a few hours old.

In addition to the period of the prodromal rash already spoken of, measles are sometimes mistaken for small-pox; the lesions in measles differ from the early papules of small-pox in their lack of induration, their remaining papular, and their method of spreading, as they have a tendency to occur together and to arrange themselves in corymbic groups with alternating paler spaces.

In certain cases the diagnosis between hæmorrhagic measles and hæmorrhagic small-pox cannot be made at once.

These with certain drug eruptions, arising from iodide of potassium, copaiba, etc., comprise about all the diseases that can be confounded with small-pox, except one, and that is varicella which often presents many difficulties. Hebra and Kaposi always maintained that the two diseases were identical, but although this is not sustained by authorities in general, it only proves that in many epidemics there is great similarity between them. Let us for a moment consider the pathology of the two diseases.

In small-pox the lesion is in the stratum mucosum, the localized œdema giving the characteristic shot-like hardness to the papule, and then as the vesicle is formed the cell-remnants are pushed together, forming many septa, dividing

the vesicles into pockets and forming the so-called multilocular vesicle which at a certain stage cannot be completely emptied by a single pin-prick.

In varicella the lesion is superficial, the delicate septa even if formed at all are rapidly destroyed, and the vesicle is unilocular and can be completely emptied.

Notice should be taken first of the prodromal symptoms usually slight in chicken-pox and of more significance in small-pox, the sudden drop of the temperature to normal in the latter simultaneous with the outbreak of the eruption, while in varicella the temperature is not increased until the rash appears and then usually rises one or more degrees.

The location of the rash—on the skin in varicella, under the skin in small-pox, soft in one, indurated in the other, rapidly developing in the former into vesicles, passing more slowly through the macular, papular, into the vesicular stage in the latter. In varicella the eruption often comes out in successive crops in the same region, while in small-pox there is uniformity of appearance, that is, papules, vesicles and pustules not being present at one time in the same locality. In small-pox the eruption appears almost always first upon the face, in varicella more often the trunk or chest. Umbilication is usually present at some stage in small-pox, in chicken-pox if present is limited to a minute area at the top of the vesicle.

Such in brief are the differential points. Corlett, in his excellent work on "Acute infectious exanthemata," from whose plates and text the writer has received much aid, says: "During the early part of an epidemic or in sporadic cases there are few physicians who may not be thrown off their guard and fail to recognize a mild form of variola. Especially is this the case when mild initial symptoms are followed by an eruptive stage in which but two or three lesions are present or when the eruption, likewise following the mild initial stage, goes through the various stages of de-

velopment in quick succession, the whole process being complete within ten days or a fortnight."

As we learn more from errors than from correct diagnosis, two cases may be of interest.

In the epidemic in Bristol County among the French-Canadians in 1900, the eruption took on a peculiar form. The first case reported in New Bedford was in a man who had been sick for about a week, who had called in no physician. He was found with a bullous eruption practically covering the whole body. Two children in the family were playing in the adjoining garden covered with superficial crusts. They had been a little "feverish" about two weeks before, and their mother said they had spots like the father. It seemed as if the children had had varicella, and as if the father was having small-pox. The services of a gentleman from out of town well-posted in contagious diseases were secured, and he diagnosed this case as well as others which developed in the neighborhood, as chicken-pox, for the following reasons,—1st, the large size of the vesico-pustules, many being the size of a two-cent piece or even larger. 2d, the absence of marked induration around them. 3d, the contents being sero-purulent rather than purulent. 4th, the utter collapse of the lesion on a pin-prick, showing them to be at this stage unilocular; and lastly the very mild constitutional symptoms present when the eruption was so universal.

This proved afterwards to be the usual form taken by the disease during this epidemic, and was true small-pox in a modified form. Out of 175 cases at this time, nearly all unvaccinated, there were only three deaths, and yet it was a common sight to find in many cases the eruption so universal and thickly scattered that there was scarcely an area of unaffected skin large enough to put the finger-tip upon. It seemed to the writer that the reason of the vesicle being unilocular was that in these cases the evolution from papule

to vesicle, and again to vesico-pustule, was so rapid that the usual septa did not have time to form, or if they did were so weak that they became almost immediately destroyed.

The writer was called in recently to see a case of a girl aged ten who had had an eruption for ten days. Her mother said the child had been vaccinated twice within the previous twelve months, though unsuccessfully. This was probably true, as the public schools demand such vaccination. About ten days before, the child had not felt well for a day or so, and had an eruption of little "blotches" as the mother expressed it. These had dried up, and there remained at the time of my visit a number of flat lesions about the size of a pea well distributed over the body, no one area especially being involved, and covered with slight scaling. There had been no small-pox in this section of the city for over a year, and no chance of contagion which could be traced. No one else in the family was affected. Taking into consideration the history of the case as far as could be ascertained, the writer diagnosed the case as one of probable chicken-pox, and diagnosed it chiefly on the lack of induration of the fast-fading lesions. A fellow-practitioner who has had considerable experience with small-pox saw the case independently on the same day, and decided the disease had been chicken-pox, and based his diagnosis on the total lack of induration. Ten days later we were called to the same house, found two children with slight papular rash and a third with a well-defined case of small-pox. It is such a case as this that shows the importance of following up closely all cases where there may arise even suspicion that small-pox may have been present.

In conclusion, it has been a source of comfort to the writer, and may be to some of his fellow-practitioners, to read Osler's words on small-pox, printed in his edition of 1901. He says: "There may be epidemics, so mild in character that it must be confessed that the recognition of the nature of the

outbreak is sometimes only confirmed by the appearance of a genuine case of the confluent or of the hæmorrhagic form."

DISCUSSION.

Dr. A. N. BROUGHTON, of Jamaica Plain: There is one thing which has been emphasized as characteristic of small-pox about which I am in doubt. It has been stated that in any given area all the lesions are of the same type. For example, if on the forehead there is a papular eruption, they are all papules, and no pustules. In a case which I saw a few weeks ago, the patient had a temperature of 103, and a universal eruption which would make a perfect picture of syphilis. This eruption in the mouth as well as in the body was entirely papular, with the exception of two distinct vesicles on the forehead and these appeared to me to be umbilicated. On the following morning I saw the case with an officer of the Board of Health, who thought it was probably not variola, especially as there were several vesicles on the abdomen, back and forehead, and yet, everywhere else were still papules. That afternoon Dr. Shea sent him to the hospital as a case of variola. I would like to ask Dr. McCollom if that statement is correct,—that the lesions in a given area are all uniform?

Dr. J. H. MCCOLLOM: In reply to Dr. Broughton's question, I will say that sometimes we do see on the abdomen, or in the inguinal region, or in the axilla, a pustular eruption, when the rest of the eruption is vesicular. There is a great difference between modified small-pox and the unmodified variety.

Dr. T. B. SHEA, of Roxbury: I think something might be said in regard to varicella. Dr. Bullard said that this is a disease occurring essentially in children. During the past week I have seen several cases of varicella in adults. The text-books pass this disease over very lightly by saying that it is a disease of childhood. Varicella is not a light thing in adults. The history looks a good deal like small-pox.

The eruption does not appear for twelve or fourteen hours, and sometimes it commences on the forehead as does small-pox. Now, as to the question of the eruption of small-pox in the beginning always being found on the face. That statement is not borne out by the experience we have had this past winter. I have seen about 1300 cases since last August. In the early cases, I usually look for the eruption on the abdomen. Also, another place where the eruption may be seen very early,—on the arch of the foot. You will often find a few papules occurring there before they occur on the face. No matter how the disease has been modified by vaccination, I think something will always be found there.

ARTICLE IX.

LEGISLATION WITH REFERENCE TO SMALL-POX AND VACCINATION.

By SAMUEL W. ABBOTT, M.D.
OF NEWTON CENTRE.

For more than thirty centuries the necessity of requiring mankind, by means of legal enactments, to observe certain rules of health, has been recognized by every civilized government.

"This necessity has found expression in those statutes which have had for their object the prevention of disease, and the result has been that those people who have most carefully protected themselves by strict legislation, and who have most rigidly enforced the observance of health laws, have enjoyed the greatest immunity from the ravages of plagues and pestilence; while a neglect of proper precautionary measures has usually resulted in periodical decimation of the population by the scourge of communicable disease."*

The foregoing extract forms a fitting introductory to the observations which I shall present, relating to the subject of legislation in its relation to small-pox and vaccination. Froude says of legislation in general: "Our human laws are but the copies, more or less imperfect, of the eternal laws, so far as we can read them, and either succeed or

* Parker and Worthington. The Law of Public Health and Safety.

promote our welfare, or fail, and bring confusion and disorder, according as the legislator's insight has detected the true principle, or has been distorted by ignorance and selfishness."

In the early history of the Province of Massachusetts, small-pox was the one disease which caused, among the people, the greatest degree of anxiety and alarm. Notwithstanding the sparsely settled condition of the population, its appearance was frequent, spasmodic and alarming. It had a way of swooping down upon the population like a destroying angel at frequent intervals, attacking nearly every man, woman and child who did not already bear upon their own bodies the scars of a previous epidemic.

In 1721, 6006 persons in Boston were ill with small-pox, or more than half the entire population, and 850 of these died. In 1792, 8346 had the disease. In this latter epidemic, it is stated that, out of a population of 19,484, 10,655 had already had small-pox, and that only 221 persons escaped having the disease, the remainder having moved out of town.*

So virulent did it at times become as to compel the Legislature to hold its sessions in some other town.†

There can be little wonder, then, that legislatures should have sought relief from this unfortunate condition by enacting such legislation as seemed necessary for the protection of the people. Many of the statutes of Massachusetts, now in existence, bearing upon this subject, date from 1701, 1776, 1792 and 1797, and are the outcome of serious epidemics. Then came the fortunate discovery of vaccination, with further legislation of 1809, and later enactments upon this same subject.

Let us now briefly review the existing legislation of Massachusetts and see what has been accomplished for the

* Report of State Sanitary Commission of 1850, p. 70.
† Report of State Sanitary Commission of 1850, p. 64. See also Stone's Life of Sir William Johnson, Vol. 1.

protection of the community. How does this legislation affect the management of epidemics of small-pox?

I shall treat the subject under two general heads: Laws relating to Small-pox, and Laws relating to Vaccination.

SMALL-POX.

1. NOTIFICATION.—First, and most important, is the law requiring the attending physician to give notice of a case occurring in his practice ("a person whom he is called to visit") *immediately*, to the local Board of Health. (Revised Laws, Chap. 75, §50.) This notice must be "in writing, over his own signature"—a mere verbal statement, delivered upon the sidewalk, or over his carriage-wheel, is not sufficient in the eye of the law. It may be urged that the young and inexperienced physician, sometimes also older and more experienced men, may not be able at the first, or even at a later visit, to distinguish between small-pox and some other disease; for example, chicken-pox or measles. In such cases, where there is even a suspicion of small-pox, it is best to give the community the benefit of the doubt, and to call in an expert who has had a large experience in the diagnosis of small-pox. During epidemic prevalence of small-pox, as at the present time, the presumption in each doubtful case (of its being small-pox) is greater than at other times. The English Notification Law differs from most American statutes upon the subject, in allowing the general practitioner a fee of 2s. 6d. for each legal notice of a case of infectious disease.

2. The same duty of notification is also incumbent upon the householder (Revised Laws, Chap. 75, §49), but, except in flagrant cases, it has not been the custom to enforce this law to the same extent as the former, chiefly from the fact that laymen cannot be expected to be as familiar with diagnosis as the trained physician. In all cases, however, of wilful concealment, when there can be no doubt as to

the knowledge of the householder, it would seem proper to take legal action.

3. The local Board of Health is also required to give "*immediate* information of each case to the School Committee." (Rev. Laws, Chap. 75, §51.)

4. When the local board is informed of the existence of a case of small-pox, it must also, "*within 24 hours*, give notice of the same to the State Board of Health." Chap. 75, §52.

5. The State Board of Health must then "*forthwith* transmit a copy of the foregoing notice of the local board to the State Board of Charity." Chap. 75, §52.

Here, then, are five separate kinds of notice required by law in each case of small-pox.

(1) By the physician to the local Board of Health.
(2) By the householder to the local Board of Health.
(3) By the local Board of Health to the local School Committee.
(4) By the local Board of Health to the State Board of Health.
(5) By the State Board of Health to the State Board of Charity.

The first four are sanitary measures intended for the protection of the public health, and the fifth is for the protection of the public treasury.

Each of these forms of notice applies with equal force to other dangerous diseases as well as to small-pox.*

What is the next provision of the law, the case having been properly notified?

6. If circumstances require it, the patient may be re-

* The English law has a certain degree of elasticity by allowing local option, by which a district may include, for the purposes of notification, certain other infectious diseases of minor importance not embraced in the general act, such, for example, as measles and whooping-cough.

The German law (1893) requires notification of cases and deaths of Asiatic cholera, typhus, yellow fever, plague and small-pox, and of cases only of typhoid fever, diphtheria, croup, recurrent fever, dysentery and scarlet fever. (Hime, Practical Guide to the Public Health Acts, 1901, p. 156.)

moved from his dwelling by the board of health "within the limits of the town." (Revised Laws, Chap. 75, §86.)

7. The board of health is required "to provide a hospital or place of reception, and such nurses and other assistance and necessaries, as is judged best for his accommodation and for the safety of the inhabitants." (R. L., Chap. 75, §42.)

If the patient lives in a spacious house, with large apartments, and especially if the house is in an isolated position, there can be no objection to his being treated in his own dwelling, provided that the health of the community is not likely to be endangered thereby. In a densely settled locality, and in small and crowded tenements, however, the public safety demands removal to a hospital.

It is quite plain moreover, that such conveyances should be used and such precaution taken in the removal as shall also least endanger the community.

The following decisions will be found useful for reference in connection with this law:

Springfield v. Worcester, 2 Cushing 52,
Spring v. Hyde Park, 137 Mass., 554,
Brown v. Murdock, 140 Mass., 314,
Hersey v. Chapin, 162 Mass., 176.

The following rules for the guidance of local boards of health in dealing with cases of disease dangerous to the public health may be deduced from the foregoing decisions:—

In case it is desired to isolate the sick person, he should be removed to a hospital to be provided by the board, except in the case where he cannot be so removed without danger to his health.

Even where the condition of the sick person is such that he cannot be removed without danger to his health, the board has no authority to establish the place where the sick person is as a hospital, without the consent of the owner thereof.

In case the board desires to provide a hospital to which to remove such cases or to establish as a hospital the place where the sick person is, and is unable to hire the building for that purpose, it should procure the issuance of a warrant under Sect. 46. R. L. Chap. 75.

8. By an old law there is also provision for the retention

of the patient where he is, "if he cannot be removed without danger to his health," and the "persons in the neighborhood" may be removed. (Revised Laws, Chap. 75, §42.)

9. In addition to the foregoing provisions of the statutes, there are still remaining a half-dozen sections relating to small-pox enacted in 1797, providing methods of a more or less clumsy character for removing sick persons, securing infected articles, impressing men to act as guards, &c. The unusual safe-guards contained in these Laws are provided in order that private property may not be appropriated to public uses without the consent of the owner, except by due process of law prescribed by the statutes.

10. When a dangerous disease "exists in a town" it becomes the duty of the board of health to "use all possible care to prevent the spreading of the infection, and to give public notice of infected places to travellers by displaying red flags at proper distances, and by all other means which in their judgment shall be most effectual for the common safety."—(Rev. Laws, c. 75, §43.) This latter clause is sufficiently broad and liberal in its terms to allow all reasonable methods to be employed which the circumstances of each individual case may require. A penalty makes it possible to enforce this law against any one who wilfully interferes with its operation.

11. HOSPITALS.—Several provisions now exist in the statutes of Massachusetts for the establishment of hospitals either temporary or permanent, for the reception of persons ill with small-pox. The emergency has arisen more urgently in the present epidemic, than ever before, in consequence of the increasing density of the population. These laws date from a period as early as 1701, when towns were authorized to establish and keep themselves constantly provided with such hospitals "subject to the regulations and orders of the board of health." (R. L., Chap. 75, §35.) A law of 1764, providing that "no such hospital shall be established within

100 rods of an inhabited dwelling house, situated in an adjoining town, without the consent of such town" (R. L. c. 75, §37), if strictly applied at the present day, would in certain densely settled cities of small area, prove a hardship, since it would limit the position of the hospital to a very small space in the centre and populous portions of such city.

By an act of 1870, the use of a building as a hospital may be prevented by a penalty, if "prohibited by the mayor and aldermen or selectmen." (Rev. Laws, c. 75, §38.)

A hospital of this character, once being established, "the physician, nurses, attendants, the persons sick therein, and all persons approaching, or coming within the limits thereof, and all furniture, and other articles used or brought there, must be subject to such regulations as may be made by the board of health."—(R. L., c. 75, §36.)

By a law enacted last year (Chap. 171 of the Acts of 1901) it was provided that "Every city in the Commonwealth shall establish within its limits, and keep itself constantly provided with one or more isolation hospitals for the reception of persons having small-pox, or any other disease dangerous to the public health." Such hospitals are to be subject to the orders and regulations of the Board of Health. (R. L., c. 75, §40.)

By further provision the city is liable to a penalty of $500, if it refuses to provide such a hospital "after having been requested so to do by the State Board of Health"—(c. 75, §40). There are in Massachusetts at the present time about fifty cities and towns, each having over 10,000 inhabitants. By a report of the State Board of Health of 1899, it appears that about half of these places only had any provisions whatever for the reception of persons ill with small-pox, and of these several were merely old-time "pest-houses" so-called, which were inadequate to fulfil the demands of an epidemic of such proportions as the past season has witnessed. This epidemic has stimulated a few more

cities to make provisions of this character, but there are still several cities and large towns which are entirely unprovided for such an emergency. The principle of waiting for an epidemic before taking action, is too much like that of waiting till a great conflagration has burned up several houses, before adequate fire apparatus is purchased by a city or town.

In the case of several of the crowded cities and towns of the metropolitan district, having a very small area, an arrangement could be made, such as is permitted in English towns, whereby two or three small contiguous cities or towns might unite in erecting and maintaining a hospital to be used in common by such municipalities.

12. The only statute in Massachusetts having reference to the treatment of the body of a person who has died of small-pox is that which forbids the transportation of such bodies by railway corporations or others, until these bodies have been "so encased, and prepared, as to preclude any danger of communicating the disease to others, by its transportation."—(R. L., Chap. 78, §43.)

Stringent laws relating to the methods of conducting funerals and to the transportation of dead bodies or persons who have died of infectious diseases, exist in some States. I am inclined, however, to agree with Dr. Chapin,* who says, "the danger to be apprehended from such bodies has been much exaggerated." Such a body, however, having once been placed in its casket or coffin and the lid closed, the coffin should not again be opened for any purpose.

The real danger in public funerals is not so much the corpse lying still in its casket, as the lingering infection transmitted by the living person to the rooms which he has occupied before his death. As a "living dog is better than a dead lion," so one live man, sick with an infectious disease, living and breathing and reproducing the *materies morbi* of such disease is, under ordinary circumstances, a

* Municipal Hygiene, p. 470.

far greater danger to the community than the dead body of one who has died of the same disease.

The following notes relating to Court decisions upon the foregoing topics may be found useful. While some of them relate to laws of other States, many of these laws have been copied from Massachusetts laws, and are of similar character and phraseology.

Indigent sick persons, suffering from infectious diseases, may be sent to a public or private hospital to be treated at the public expense.

State v. New Orleans, 27 La. Ann. 521.
Tucker v. City of Virginia, 4 Nev. 20.

A person sick with an infectious disease in his own house, or in suitable apartments at a hotel or boarding-house, cannot be removed without his consent, under the authority conferred by a city charter "to abate and remove nuisances."

Brown v. Utica, 2 Barb. 104.

For further decisions relative to taking possession of houses and furniture without consent of owner, for use as a hospital, &c., see,

Spring v. Hyde Park, 137 Mass. 554.
Brown v. Murdock, 140 Mass. 314.
Lynde v. Rockland, 66 Maine, 309.

Health authorities can decide on propriety of removal,

Haverty v. Bass, 66 Maine, 71.

"In order to carry out the law relating to the isolation of persons suffering with small-pox, and for the protection of the community, it may become necessary to convert private houses into hospitals and make them subject to hospital regulations, to seize and destroy infected articles, and to do many other things, which under ordinary circumstances would be considered a gross outrage upon the rights of persons and property. This is allowed, on the same principle

that houses are allowed to be torn down to stop a conflagration. *Salus populi est suprema lex*, is the governing law in such cases. When the public health and human life are concerned, the law requires the highest degree of care. It will not allow of experiments to see if a less degree of care will not answer." (Parker & Worthington, p. 134.)

Seavey v. Preble, 64 Maine, 120.
Labrie v. Manchester, 59 N. H. 120.
Farmington v. Jones, 36 N. H. 271.
Kennebunk v. Inhabitants of Alfred, 19 Me. 221.

"Notwithstanding this privilege, it does not appear that a house may be seized and occupied against the will of the owner. The consent of the owner must be obtained to the use and appropriation of his property by the board of health, and provision made for his compensation, by means of a regular contract. Otherwise the property must be appropriated to the use of the board of health by proceedings regularly had for its impressment to the public use, as is provided for in the statutes of several States, accompanied by suitable safeguards in each case."

Lynde v. Rockland, 66 Maine, 309.
Spring v. Hyde Park, 137 Mass. 554.

In the matter of disinfection after small-pox the law of this State (Rev. Laws, Chap. 75, §49) states, "Upon the death, recovery or removal of such person, such of the rooms of said house, and such of the articles therein, as, in the opinion of the board of health, have been subjected to infection or contagion, shall be disinfected by the householder, to the satisfaction of the board of health."

Practically this work is done by the board of health, but strict compliance with the law would at least in all such instances make it advisable to give the owner timely notice that such rooms and articles are to be disinfected within a certain time, and if the owner fails to comply within the specified time, the board is fully justified in doing the work at the householder's expense. (Parker & Worthington, p. 139.)

VACCINATION.

In nearly all States, as well as in civilized countries generally, laws exist providing for the vaccination of the population, and particularly for that portion which attends the public schools.

Abeel v. Clark, 84 Cal. 226.

This requirement, of submission to vaccination, is a constitutional exercise of the police power of the State, which can be sustained as a precautionary measure in the interest of the public health. (Parker & Worthington, p. 139.)

The Massachusetts Laws relating to vaccination require—

1. "A parent or guardian who neglects to cause his child or ward to be vaccinated before the child or ward attains the age of two years, except as provided in R. L., Chap. 75, Section 139, shall forfeit $5 for every year during which such neglect continues."

2. By an act of 1855, the local Board of Health must "require and enforce the vaccination and revaccination of all the inhabitants," if, in the opinion of the board, it is necessary for the public health and safety. Every person over 21 years old, not under guardianship, who refuses or neglects to comply with this requirement is liable to a penalty. (Chap. 75, §137.)

The chief defect in this law, as also in the laws of other states, is that it recognizes a false principle in sanitation. The ideal community, so far as vaccination is concerned, is not that population in which "the public health and safety" requires vaccination at irregular times and seasons, on the eve of, or in the midst of great epidemics, but a community in which vaccination and revaccination are constantly and uniformly practised year by year upon all young children and upon older children at the end of a definite period, say, of 10 or 12 years. In such a community there are no epidemics of small-pox, because there is no virgin soil in which it can take root and flourish.

A law, then, which only requires vaccination when small-pox is prevailing or is epidemic in the State or district, is defective, and wrong in principle, and a community living under such conditions and with such laws, may, as I have already said, be likened to one which waits for a great fire before purchasing its fire apparatus. The only correct sanitary principle in regard to vaccination is that of complete and permanent immunity, and not that of partial and temporary immunity of the people.

3. By a further provision of the same law (Chap. 75, Revised Laws, §137), as amended in 1894, the local Board of Health is required "to provide the inhabitants with free vaccination," if, "in the opinion of said board," the public safety requires it. Here, again, the law, unfortunately, recognizes a wrong principle of waiting until "in the opinion" of a local board of health "the public safety requires" vaccination. As an illustration of the defective character of this principle during the present epidemic, several instances have occurred in which local boards of health have, unfortunately, been composed (at least partially) of men who were opposed to vaccination. In several instances, the presence of one anti-vaccinationist upon a local board of health has so influenced the action of the board as to result in serious harm to the community, by delaying public vaccination, until the common sense of the community compelled them to act.

Possibly, the framers of such laws recognized this defect, but also recognized the common but often fallacious principle of local self-government, which allows those municipalities which do not act for the common safety of their population, to suffer the consequences of such neglect. But, unfortunately, small-pox is a contagious disease, and is not confined to the boundary lines of cities or towns. The neglect of one town or one State may prove also a serious menace to the people of another town or State. Again,

the non-voting part of the community, the women and especially the unprotected children, may become the victims of the neglect of the voting portion.

4. For many years a beneficent law existed in this State as well as in many others, providing that the School Committee "shall not allow a child who has not been duly vaccinated to be admitted to or connected with the public schools." The law was absolute, and wherever this provision has existed and has been thoroughly enforced, it has proved to be a helpful aid in protecting the community. But, unfortunately, in consequence of unwise opposition, this law was so amended in 1894, as to admit to school any child who was, in the opinion of a "regular practising physician," "an unfit subject for vaccination" (R. L., Chap. 75, §139). The intent of this law was to admit such children as might be temporarily unfit for vaccination, in consequence of some ailment which might possibly make it improper to vaccinate the child for the time being. The serious blunder of enacting such a statute has been illustrated in the experience of the present epidemic, wherein it has appeared that the law has been frequently misapplied, by using it as a means of exemption for *any* child, without regard to its condition of health, on the ground that the certifying physician believed that all children were unfit subjects for vaccination. Worse than this, such certificates became for a time the subject of further abuse, since physicians advertised that they would, for a small fee, send certificates by mail to parties at a distance, without personal examination of the child. This mode of abuse has now been corrected by a law of this year, requiring that the certifying physician "at the time of giving the certificate, shall have personally examined the child, and that he is of the opinion that its physical condition is such that its health will be endangered by vaccination." This exemption is in force, according to the statutes, "while such condition con-

tinues." The law is still defective, since it still allows a physician to apply it to any child whom he may consider an unfit subject, and may hence apply it to all children, if he happens to be an opponent of vaccination in general. To remedy this defect, the local board of health, through its medical officer, should have discretionary power to revise the certificate upon similar personal examination.

The direct consequences of this unwise legislation are seen in the present epidemic, wherein it appears that during the twelve months ended Dec. 31, 1901, 64 unvaccinated school children, between the ages of five and fifteen years, had been attacked with small-pox. The records of 1902 have not yet been tabulated, since the year is now but half completed. But an examination of all the records for the past 14 years show that during that time 389 children under 15 years of age had been attacked with small-pox, of whom 66 were vaccinated and 323 were unvaccinated. The most significant fact, however, in regard to these 389 cases is, that out of the 66 vaccinated children there were *no* deaths and among the 323 unvaccinated children there were 37 deaths, or 11.4% of the whole.

5. Another statute requires that the "inmates" of all incorporated manufacturing companies, and those of public institutions, shall be vaccinated when the local board of health requires it. (R. L., c. 75, §138.) As a general rule, the superintendents of public institutions are careful to keep their inmates vaccinated, and, as a consequence, but few cases of small-pox have occurred among them during the present epidemic. Such, however, is not the invariable rule with manufacturing corporations. In several small manufacturing villages in the State, through neglect of this precaution, serious outbreaks have occurred.

The superintendents of the paper-mills in Holyoke and in other western towns of the State, where paper is largely made, have been careful in requiring their employés to be

vaccinated, so that the serious outbreaks among such persons which once prevailed in consequence of the handling of infected rags, have, in this epidemic, been notably absent.

In the brief summary which I have here presented, I have only touched upon some of the most important points in the legislation relative to small-pox and vaccination. Other and important questions of a legal character have arisen and may continue to arise in the future. Two points, however, are very clear:—

First. The necessity of sufficient laws in all countries, relative to infectious disease prevention.

Second. The necessity of careful enforcement of such laws. We may choose between two courses. By fulfilling these two conditions: Law,* and its enforcement, the State may be rid of small-pox, with the exception of such single cases as may be imported. On the other hand, in the absence of law, or if existing and not enforced, small-pox will run riot at intervals among the people.

In illustration of this point, I will call your attention to the condition of two countries which afford examples of careful study:—

In England, vaccination laws have existed for nearly a century, but these laws are defective and limited in character, the force of the legislation having been expended upon primary vaccination, to the serious neglect of revaccination. Added to this, opposition to vaccination has been more vigorously agitated there than in any other country in the world. So great was this opposition, that a Parliamentary Commission spent nearly eight years in considering the subject, and published a report of several thousand pages, the final result of which was the enactment of a blundering amendment, usually known as the "Conscientious Objector" Act, the weakness of which was shown upon the first

*By the term Law here, an adequate law requiring the vaccination and revaccination of the entire population above a certain age is intended.

occasion of a trial at court under this law. (See Hime's Practical Guide to the Public Health Acts, Appendix, p. 53.)

As a consequence of the opposition prevailing in England, the exemptions from vaccination existing in different cities among children subject to vaccination, have varied from as low as 3.8% in Liverpool and 6.4% in Manchester, to 79.9% in Leicester and 23.9% in London, in recent years; and what is the result of this wholesale neglect? Small-pox is prevailing in London this year to a greater extent than at any time since the serious outbreak of 1871-'72. From the first of last January until now, the new cases of small-pox reported in London have averaged over 300 per week.

Contrast with this the splendid condition of Germany to-day. That country, with its fifty millions of population, may be likened to a city surrounded with a solid and impenetrable rampart or wall, and a picket-line of well-drilled soldiers outside of the wall. This wall and picket-line are the well-enforced vaccination laws of the country. The men, women and children are the well-protected army of soldiers. Small-pox may occasionally break through the outer picket-line in the shape of some wretched, unvaccinated immigrant from Russia, Austria, Italy, France, or Belgium, but the great mass of the well-protected population inside the fortification suffers no damage. This condition was effected by the enactment of a law in 1874 providing for the vaccination of every infant and every pupil in the schools at definite ages. As might be expected, with the existence of such a law, small-pox would gradually become extinct. In the first year, only a small part of the population would be vaccinated (1, Primaries, *i. e.* children under 2 years of age, and, 2, Revaccinations, or children in their twelfth year); but the work of each succeeding year would add proportionally to the vaccinated popu-

RECORD OF VACCINATION.

To be filled on the day of Vaccination.

RECORD OF VACCINA

To be filled on the day of Inspectio

1	2	3	4	5		6	7	8	9	10	11		12	13		1
Number.	Date of Vaccination.	Name of Vaccinated Person.	In case of Re-vaccination at ages over ten years, of persons previously successfully vaccinated, mark R.	Age.		Residence. (Street and Number.)	Source of Lymph. Name of Producer.	Number of separate scarified areas or punctures made.	Initials of Vaccinator.	Date of Inspection.	Result.		Initials of Inspector.	Certificate of School Vaccination given.		REMA
				Yrs.	Mos.						Successful number of separate vesicles produced.	Unsuccessful.		Yes.	No.	
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CCINATION. R

of Vaccination.

6	7	8	9	10	
Residence. (Street and Number.)	Source of Lymph. Name of Producer.	Number of separate scarified areas or punctures made.	Initials of Vaccinator.	Date of Inspection.	Successful marks

RECORD OF VACCINATION.

To be filled on the day of Inspection.

10	11		12	13		14	
	Result.			Certificate of School Vaccination given.			
Date of Inspection.	Successful number of separate vesicles produced.	Unsuccessful.	Initials of Inspector.	Yes.	No.	REMARKS.	
							1
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lation. This has been the result, and at the present day no cases of small-pox occur in that country which are not imported from neighboring lands, outside the limits of Germany. Such might be the case with any State or nation which adopts a similar course; and if all countries were to adopt it, small-pox would become a thing of the past. It is sometimes argued that small-pox is a filth disease, and only prevails among filthy people. But Paris and Vienna are as clean as Berlin, and yet these cities are very often visited by small-pox, while Berlin has none.

In one important point, our own laws as well as those of most American States are defective, and that is in the absence of any definite provision requiring a record of vaccination to be made, at least, in the case of all vaccinations performed under public authority. Boards of health often make wholesale vaccinations upon the population of large districts, institutions, schools, the employes of corporations, etc., without even a slight record of the names only of those who have been vaccinated. The laws, or at least the regulations of other countries are very explicit in this matter and require not only a record of the date, name, age and sex of each person vaccinated, but also a record of the result of the subsequent inspection, which should always be made at the end of about a week. Such a record is often of very great value in settling disputed cases. To meet this objection, the State Board of Health has furnished to every local board in the State, suitable record-books for this purpose. These books are modelled upon the forms supplied by the Local Government Board of England to the public vaccinators.

Herewith is a copy of a single sheet, each of which gives opportunity for recording 10 vaccinations and each book for 1000 or 2000, according to size of the book.

In this paper I have purposely avoided allusion to those vexing questions relating to the economic side of this question which invariably arise in every epidemic of small-pox.

I have omitted discussion of this phase of the subject, both for want of time, and because the valuable time of a scientific body can be better employed than by the discussion of financial matters which can quite as well be submitted to the lay members of boards of health.

Those health officers, however, who desire to consult the various statutes and decisions relating to the expenses of caring for small-pox patients, the salaries of attending physicians, nurses, attendants, liability for destruction and injury of houses, furniture, bedding, etc., etc., may find abundant reference in the legal works upon the subject. See Parker and Worthington, Law of Public Health and Safety; and the following Supreme Court Decisions:

Spring v. Hyde Park, Mass., 137, 554.
Brown v. Murdock, 140 Mass., 314.
Orono v. Peavey, 66 Me., 60.
Hampden v. Newburgh, 67 Me., 370.
Farnsworth v. Kalkaska Co., 56 Mich., 640.
Rae v. Flint, 51 Mich., 526.
Wilkinson v. Long Rapids, 74 Mich., 63.
Clinton v. Clinton Co., 61 Iowa, 205.
Kellog v. St. George, 28 Me., 255.
Miller v. Somerset, 14 Mass., 396.
Mitchell v. Cornville, 12 Mass., 333.
Childs v. Phillips, 45 Me., 408.
Gill v. Appanoose Co., 68 Iowa, 20.
McIntire v. Pembroke, 53 N. H., 462.
Albany v. Wilkinson, 28 N. H., 9.
Fort Wayne v. Rosenthal, 75 Ind., 156.
Staples v. Plymouth Co., 62 Iowa, 364.
Elliott v. Supervisors, 58 Mich., 452.
People v. Macomb. Co., 3 Mich., 475.
Schmidt v. Stearns Co. 34 Minn., 112.
Labrie v. Manchester, 59 N. H., 120.
Farmington v. Jones, 36 N. H., 271.
Kennebunk v. Alfred, 19 Me., 221.

One of the unfortunate circumstances connected with sanitary legislation at the present day is the absence of medical men in the Legislature. On this account, Public Health Committees must necessarily be composed either wholly or

almost wholly of laymen. Medical questions are constantly arising at every session, and it is important that a committee which is called upon to discuss and present to the Legislature many medical and sanitary questions should have at least one medical member. Laymen do not appreciate the value of legislation intended for the prevention of disease so keenly as men who are trained for this special purpose. Hence it is important that this want should be supplied, in order that the efficiency of sanitary legislation may not be impaired.

DISCUSSION.

Dr. G. A. MILES, of Somerville: I would like to speak of one thing to which Dr. Abbott refers, and that is with reference to children getting into our schools unvaccinated, not only by certificates given by physicians, certifying that the child is an unfit subject to be vaccinated, but also those children who have been vaccinated, but no inspection of them afterward to see the effect. Since this outbreak in Somerville I have been surprised to find the number of children in school holding certificates of vaccination, although the operation was entirely unsuccessful. This has brought very forcibly to my mind the great importance of examining every case before a certificate is given, not only to see that the patient is protected, but also that it is being properly cared for and doing well.

Dr. S. H. DURGIN, of Boston: I think for a physician to give a certificate before he knows that the vaccination has taken is both stupid and illegal, and it should be denounced from one end of the land to the other.

ARTICLE X.

SMALL-POX: ITS MEDICAL TREATMENT.

By MICHAEL KELLY, M.D.
OF FALL RIVER.

Small-Pox is a disease which affects the entire system. The physician's duties in regard to it are many and varied. Its treatment is necessarily expectant and symptomatic. A self-limited disease, running a definite and regular course, no plan of treatment will abort or modify it when once developed. Perhaps its present extensive spread, giving opportunities for its study by numerous observers using modern scientific methods of research, will soon disclose the nature of the contagium, and thus rob it, in those who through ignorance or misfortune have escaped vaccination, of its loathsomeness and terrors.

As soon as the diagnosis is made, if not as soon as a competent observer has a reasonable suspicion that a case is variola, the patient should be isolated. It would be well, of course, to have some suitable place of detention for cases that are only reasonably suspicious. For the treatment of those in whom the disease is diagnosed, an isolation hospital is the proper place, and there they should be sent at once.

The isolation hospital, where patients are to be confined from two to three weeks in mild cases, and from four to five weeks or more in severe cases, should be in construction

and equipment thoroughly on a par with the times. It should be a retreat in which the physician himself would elect to be confined, or to which he would willingly send members of his family or his friends if necessary. Small-pox patients are not criminals nor outcasts, and should not be treated as such; they are truly to-day either the victims of unfortunate circumstances or of their own invincible ignorance; and in submitting to the quarantine lawfully imposed by the Boards of Health they are making a sacrifice of their feelings at least for the public good. They should have all the comforts which their unfortunate condition demands.

As these patients cannot have the physicians of their choice but only those selected by the city or State, the physicians so selected should be men about whose qualifications there can be no reasonable doubt—men willing to prescribe, superintend or nurse as occasion demands—men who will inspire assistants and subordinates to discharge their duties with incessant vigilance. The disease, besides being a danger and annoyance to the patient, is a source of greater danger, embarrassment and expense to the community, and the physician entrusted with the mananagement of it should be fully able to sustain his combined grave responsibilities to patient and public.

Strong, ambitious, trained nurses, recently vaccinated and not afraid of the disease, should be secured when possible. There is considerable lifting, changing of position in bed, daily changing of bed clothing and bathing to be done for these patients, which would soon prove too much for delicate or timid nurses. The urgent need of keeping severe cases under unremitting observation, and the fact that death may occur suddenly at any time during the disease, even in cases apparently doing well, should be plainly taught the nurses from the start. Male nurses, when available, are better for men.

Physicians, nurses and all persons who may come in slightest contact with patients or their belongings, should be vaccinated, without regard to the time of previous vaccination, before beginning the discharge of their duties.

I believe it is within the province of this paper to say a word about prophylaxis. After the experience of a century we can declare without any hesitation that the head and front of the prophylaxis is vaccination. The disease would soon cease to exist if vaccination were universally practised.

An experience in managing an outbreak of the disease in Fall River, in 1899, proved conclusively a truth which theretofore I had accepted on the authority of teachers and text-books. Of a family of fourteen persons under my care two did not contract the disease, and these two were the only members vaccinated. These two always mingled freely with the family, and were sent to the hospital for isolation only. In all the families, nineteen in number, were members who escaped the disease, and these were invariably persons previously vaccinated. An old lady fifty-five years of age, the last of a family of six to yield to the disease, had been vaccinated, one cicatrix only, thirty years before. Though in very feeble health, for which she was under a physician's care, she developed only a fairly severe varioloid, and made a rapid recovery. Two, who were vaccinated ten years previously, each showing only one shadowy cicatrix, had well-marked varioloid but were not seriously ill at any time. None of my cases of varioloid, vaccinated at a nearer date than ten years before, manifested the disease in anything like a severe form. I have seen no case of varioloid in which there was more than one cicatrix from a previous vaccination. From the history of these cases, and others which I might mention, it is evident that vaccination properly performed and repeated at suitable intervals is the corner-stone of the prophylaxis.

Varioloid would neither be very annoying to the patient,

nor demand much attention from the physician, were it not for the position in which it places the patient in relation to the public. The fact that he who harbors varioloid is as apt to spread the disease in its worst form as he is who has variola vera, makes it as necessary to observe the strict laws of quarantine, disinfection and fumigation in the one case as in the other.

Only in rare cases do the subjects of varioloid require more than a day or two in bed. Owing to the effects of the preceding vaccination, remote though it may have been, as in the case just mentioned, in which it had been done thirty years previously, varioloid has no complications. After the the fourth or fifth day of the eruption, the low temperature which may have existed returns to normal, the vesicles or abortive pustules dessicate without progressing into fully matured lesions, or their contents become absorbed, leaving only broken shells. From this out the patients with this mild form of the disease are difficult to control; it is difficult at any time to convince them that they have a disease which is a menace to the public; it is hard to keep them quarantined until by efficient disinfection and fumigation the dangers of their spreading the disease have passed. The difficulties and uncertainties attached possibly or necessarily to the proper performance of the needed disinfection and fumigation in private houses point directly to the imperative need of treatment of even these mild cases in a hospital.

In discrete and confluent small-pox the indications for treatment are to preserve the patient's strength by every possible means from beginning to end, and to render the manifestations in the skin as little annoying as possible.

No special treatment is indicated for the incubation period except to prevent patients from doing anything which might debilitate them for the disease about to develop. I made attempts in two cases, however, to modify the oncoming disease at this stage. Two patients, a young man aged

twenty-two years and an infant of seven weeks, I vaccinated in what from the subsequent history proved to be the sixth and fourth days of this period respectively. Though the young man came down with the disease ten days later than a younger brother, both previously unvaccinated, he had the disease in a much milder form than his younger brother, and was discharged a week earlier. The infant developed a very abundant crop of discrete eruptions, had a high temperature, and skin very swollen and œdematous, and though the text-books inform us that it is the rule for children under one year with either discrete or confluent variola to die, this child was well at the end of twenty-one days, and had no pitting after five weeks. I will not say that these results were a consequence and not a coincidence of vaccination, or that the striking result in the case of the young man was not due to individual peculiarity of constitution. The cases are too few for a general conclusion. They are mentioned simply because they may assist in the discussion which is still *sub judice* of the benefits of vaccination in the incubation stage or at least in the early or middle period of that stage.

In the stage of invasion, occurring on the tenth to the fifteenth day of the incubation period, rest in bed is indicated. The rooms should have an abundance of pure fresh air. The absence of fresh air in the small, close rooms of tenement houses is more oppressive to these patients than it is to the subjects of typhoid fever or other febrile diseases. Twenty-five cases treated for a week in the eruptive period in tenement houses, and all then removed in one forenoon to a hospital where there was an abundance of fresh air of a suitable temperature, convinced the writer that fresh air is absolutely indispensable in keeping down the mortality. In these cases an imminent mortality of seven to ten per cent. was converted into no mortality at all. The temperature of the room should not be above 70 Fahr. nor much below 60 Fahr.

Solar light should be excluded as completely as possible, since it is reasonably certain that its access to the face is largely productive of the pitting left after severe cases. Inexpensive dark window curtains will serve this purpose. The headache, backache and other distressing symptoms following the rigor of this stage yield readily to four or five grains of phenacetine guarded by a little sherry wine or brandy, and repeated every two or three hours according to indications. The same formula, supplemented by moderate doses of the bromides and chloral, is useful for the fever and delirium accompanying the period of pustulation and suppuration. Gastric irritability is relieved by sucking small pieces of ice or by taking small sips of iced lemonade. Poultices or rubber bags filled with hot water applied to the stomach or back to relieve the distressing symptoms in these parts are contraindicated, as such applications make the effects of the oncoming eruptions more marked and permanent in the parts to which they are applied.

The disease is profoundly exhausting, and tonics are indicated at this stage and throughout its entire course. Only easily digestible and assimilable yet nutritious food should be offered. Milk, eggs, animal broths, and abundance of pure cold water or lemonade are very acceptable. Mild laxatives may be given in the early stage, and if continued at intervals with care throughout the disease may prevent the constipation which sooner or later may appear as a complication. Stimulants in the forms of sherry or port wines, whiskey or brandy, in free doses and repeated every two or three hours, especially in the afternoon and night, are useful and agreeable adjuvants to the sustaining treatment at any period of the disease. The general treatment adopted in this stage is continued to the end with but little essential change. Complications at any stage are successfully treated upon general principles.

Pain and tension in the skin, and the itching which de-

velops as the eruption becomes vesicular and pustular, are relieved by the application to the parts of cloths wrung out of cold or hot water according to the patient's choice, and frequently changed. Evidently it is the moisture of the applications which produces the soothing effect. As this stage advances, the nose, mouth, soft palate and throat suffer from the eruption, and deglutition becomes difficult and painful. Gargles of chlorate of potash when the patient can use them are of benefit, but most marked relief is obtained from a careful rinsing of the mouth by the nurse with a piece of clean old linen wrapped around her finger and previously dipped in a one to twenty per cent. solution of boric acid. For the same purpose a saturated solution of boric acid in rose water may be used in an atomizer and sprayed frequently over the parts. This spray applied to any afflicted part of the surface of the body is most grateful through the rest of the disease. In sprays not to be used about the nose, mouth or eyes, a solution of corrosive sublimate is useful. Some patients prefer these solutions in the form of spray, and others prefer cloths saturated with them applied to the parts and frequently changed. The intense itching in the late pustular or suppurative stage is relieved by the mixture recommended by Dr. Hare: olive oil and lime water equal parts to which a little carbolic acid or thymoline is added. This can be applied with a large camel-hair throat brush with a long twisted wire handle. These sprays and the oleaginous mixture can be used two or three times daily and in severe cases much oftener. As the disease advances they appear to hasten the falling of the crusts, exclude the air from the skin, and aid in the general work of disinfection. During the height of the secondary fever, the fever of suppuration, daily bichloride sponge baths only can be given, as the patients are too sore and too much swollen to make feasible the giving of immersion baths.

Rupture and dessication of the pustules begins on the

thirteenth or fourteenth day of the eruption. This is the period at which death is most likely to occur from toxæmia produced by absorption of some of the large quantity of pus upon the surface of the body. On account of the energetic efforts which have always been made to tide patients over this period, a treatment employed by me with apparent success in an unusually severe case of confluent small-pox may be worthy of mention. On the eleventh day of the eruption, and therefore two or three days before rupture and dessication of the pustules, patient had an evening temperature of 105.5 Fahr. Face, arms, forearms, hands, legs and feet were intensely swollen and œdematous. Patient profoundly apathetic. I injected subcutaneously under strict antiseptic conditions into the tissues of the back ten c.c. of antistreptococcus serum; two similar injections at intervals of twenty-four hours were made into the forearm and thigh respectively. In about twelve hours after the first injection the œdema about the eyelids began to subside, patient could move his eyelashes; a similar change took place about the mouth, and the profound apathy rapidly abated. The breathing and pulse also improved. In a few hours after the last injection the morning temperature was normal. It was fully sixteen days later, however, before the patient ceased to have an afternoon temperature of a degree or a degree and a half. Patient made a good recovery, and developed no abscesses nor sores at any time. I had but one case seriously ill enough to indicate this treatment; nevertheless, I consider the improvement after injections rather more than a coincidence.

When suppuration and the secondary fever have subsided, and the crusts, or at least a crop or two of them, have fallen, the patient is given three thorough antiseptic baths at intervals of twenty-four hours. Then he gets a clean, fresh suit of clothes, is removed to an uninfected part of the building or, if the weather will permit, to a tent

outside the grounds, where the antiseptic bathing is continued for three days more. Then all his belongings which it is not deemed advisable to destroy by burning are placed in a large wooden receptacle, where they are exposed to formaldehyde vapor for twelve to fourteen hours, and then the patient is dismissed. Ambulance drivers, ambulances and their effects should all be disinfected and fumigated.

In the present visitation of the disease in the United States not much of the usually fatal hæmorrhagic variety of the disease is seen. I have not seen a case. In an extensive survey of the literature of the subject, the few remedies concerning whose probable efficacy there is the greatest agreement amongst writers are turpentine and ergot in liberal doses, carbonate of ammonium, quinine, and the practice of transfusion.

In country towns, or in the country in general, where there is no isolation hospital and the patients must be confined in a private house, the general course indicated should be pursued as nearly as possible. The room in which the patient is confined should be as far removed from the living rooms as possible. Cloths or sheets kept continually saturated with a disinfecting solution should be suspended in the doorway and at the windows, especially if the windows open upon a street or upon near neighboring houses.

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DISCUSSION.

Dr. F. I. KNIGHT, of Boston: I would like to ask about the use of the red light in the treatment of small-pox. We have heard a great deal about the extraordinary effect of the red light on lessening the severity of the local process going on in the skin.

Dr. C. J. WHITE, of Boston: In regard to the use of the red light, I will say, that so far it has not proved to be efficacious in the treatment of small-pox. No detailed statement about its use in Europe has, however, come to my notice, so perhaps we should not judge definitely about its success across the water; but Dr. Councilman of Boston does not believe in it, and says he has found no benefit resulting from its use.

Dr. BANCROFT, of Boston: In regard to the use of the red light, we fitted up two rooms with the red light. We did this by fixing up curtains. I suppose there were about 20 cases there in all, and we kept them there long enough to see what influence the red light might have on the pitting. One of these cases was very sick, and it was rather a surprise to us that the patient got well, but I have seen many such cases get well. I think from the experience we have had that we cannot claim any great benefit from the red light.

Dr. Durgin spoke about the use of serum. We tried this in 8 or 9 cases which had not been vaccinated, but from these few cases we cannot conclude anything. In two cases it was tried very early, and it seemed to me it might have had some effect. I think it should be tried only in unvaccinated cases, and those cases children.

GENERAL DISCUSSION.

Dr. E. C. NORTON, of Norwood: I think there is reason for not vaccinating in some cases, and would like to ask Dr. McCollom what kind of cases he would give as not suitable to vaccinate?

Dr. MCCOLLOM: Of course there are a few cases, as Dr. Norton says, where it might not be advisable to vaccinate, but they are very few. In answer to his question as to what kind of cases, I would say, if a child is in the last stages of tuberculosis, or, if in the active stage of a very severe attack of scarlet fever, I should not want to vaccinate, and it is a question when a child is having a very severe attack of whooping-cough whether it is wise to vaccinate, although there are some men who claim that whooping-cough has been modified by vaccination. I think, however, that this latter must be an error in observation, as I can hardly see how vaccination would modify whooping-cough.

Dr. H. L. CHASE, of Brookline: I would like to say a word about the vaccination of pregnant women. In the town in which I live opinion is rather against vaccinating pregnant women. The late Dr. Henry Martin was always against doing this unless the woman was much exposed to small-pox. I should like an expression of opinion in regard to this point from Dr. McCollom.

Dr. MCCOLLOM: I have vaccinated a good many pregnant women. In a case of this kind I think it is well to tell the woman just how the matter stands. If she has been exposed to small-pox, tell her she has been exposed, and that there is danger of taking the disease, and many women, when they understand how it is, will be glad to be vaccinated. Of course there are some women who have made up their mind that they will not be vaccinated, and I do not believe in forcing them.

Dr. NICHOLS: I have never seen any harm from vaccinating pregnant women, and if there is any, certainly in this large assemblage of medical men of experience we ought to find out something about it.

Dr. KNIGHT: I would like to ask Dr. McCollom if he thinks any child who is well enough to go to school should be exempt from vaccination.

Dr. MCCOLLOM: I should say if a child is able to go to school, it is able to be vaccinated. I will say right here that I believe the only time when a child should not be vaccinated is when the child is sick with some acute disease, and will succomb to it in a few days.

Dr. J. A. DOUGLASS, of Amesbury: I should like to ask Dr. McCollom how early in the development of small-pox it is communicable to those who have not been vaccinated. Prof. Clark, of New York, stated that it is not communicable until it becomes characteristic, and he cited a number of cases that came into Bellevue Hospital without any history or symptoms of what was coming, except severe fever. They were placed in the fever ward and watched. A few days later, on coming into the ward, the characteristic eruption was observed, and the patients were immediately placed in the small-pox ward. None of the other patients in the fever ward contracted the disease.

I would also like to ask Dr. McCollom what precautions against spreading the disease he considers necessary for a doctor to take when he is attending a case of small-pox, and what he is in the habit of doing to satisfy his other patients, and the sentiment of the community in which he lives.

Dr. MCCOLLOM: In regard to the first question, I think small-pox is communicable from the commencement of the initial fever.

In regard to the second question,—as far as the doctor himself is concerned, of course he should be vaccinated successfully, and after seeing a patient ill with small-pox he should change his clothes.

Dr. C. C. FOSTER, of Cambridge: There is one class of cases that has been brought to my attention, and that is cases of chronic hip-joint or spine disease. Parents of children with these diseases are very loath to have their children vaccinated. These children do not usually go to school, and of course they are not with other children

much. I should think these would be doubtful cases. Where they are "walking-cases" I usually advise vaccination, but the parents often refuse.

Dr. McCollom: In regard to what Dr. Foster has said about these cases of hip-joint disease, I can see no reason why these patients should not be vaccinated. Of course if the children are very much debilitated that puts another light upon it.

Dr. S. W. Abbott: I believe the words, "This child has been successfully vaccinated," should be required on all certificates of vaccination.

I never saw a case that had been communicated from one person to another through the medium of an attending physician, but I did see this peculiar case: During the epidemic of 1872, I had in my stable a cow two or three years old. After seeing a case of small-pox I used to hang my coat up in the stable for the protection of other patients and my family, and that cow had the cow-pox.

Dr. S. Hanscom, of Somerville: I would like to ask Dr. McCollom how soon he would vaccinate an infant?

Dr. McCollom: I would vaccinate a child 48 hours after birth if there was a case of small-pox in the family. I have vaccinated a child 24 hours after birth. Of course it is not necessary to make a large scarification.

Dr. Broughton: I just wish to call attention to one little trick which I found extremely useful in the preparation of the arm for vaccination. After soap and water and scrubbing, one or two strokes of a razor will clean the arm splendidly. Just shave a small spot on the arm. I did this in all my cases, saw no ill effects, and was able to do it rapidly.

Dr. B. R. Symonds, of Salem: We have had a number of cases of small-pox in Salem this winter, and I should like to briefly outline our method of dealing with it, and ask Dr. Durgin if we are using about the same methods that are used in Boston. When we hear of a case we go to the house and remove the patient to the small-pox hospital.

We then vaccinate everyone in that house, and quarantine the house for two weeks. During that time we watch the house rigidly, allowing no one to go to or from the house, and we support and maintain the inmates while the quarantine is on. I would ask Dr. Durgin if he thinks this is necessary; if we could not just vaccinate everybody in the house, or all that have come in contact with the case, and then let them go, simply keeping an eye on them to see if they contract the disease, when they could be immediately removed to the hospital?

Dr. DURGIN: In Boston we have not quarantined the house, but have immediately taken the patient away, cleaned up, and vaccinated the others. After this I see no reason for quarantining the house. I believe it is important to watch those who have been exposed for fifteen days, although you would not expect them to be ill for nine or ten days.

Dr. SYMONDS: As a member of the Board of Health of the City of Salem, and appointed by the City Government, I wish to work for the city's good, and desire to keep the expenses down as much as possible, and do the work most satisfactorily. Being the only medical man on the board I did not want to take any chances. During the time a house has been quarantined we have fed all the inmates of the house, provided the quarantine officers. We have had seven cases of small-pox. I think we should vaccinate them, let them go and watch them, but so far we have vaccinated the people, quarantined and fed them, and have had but very few cases. Nearly, if not every case, has been imported from other places. Our general free vaccination helped us amazingly.

Dr. DURGIN: I am very glad to hear this good report of Dr. Symonds. The remark has been made by Dr. McCollom, during the meeting, that it would be better to call the place for small-pox patients the "Small-pox Hospital," rather than the "Pest-house." Many persons in the community think that when one enters the pest-house it means "good-bye forever." Great dread is associated with that word, and we are trying to do away with it, and are

calling the place the "Small-pox Hospital," and we are succeeding, by making it as inviting and comfortable as possible to those who go there. While we take cases away from home to the hospital, I believe we should do everything in our power to overcome this natural dread by calling it an hospital, and making the people feel that every comfort will be provided for the sick person.

In regard to vaccinating children who are not very well, it has been the custom for many years to bring such children to the Board of Health office and try to get them into school without vaccination. I have always tried to persuade parents to have their children vaccinated, and have almost invariably succeeded. In this way hundreds of such children have been vaccinated, and I have never seen any bad effects from it. I believe that this can safely be done in 99 out of every 100 children whose parents want to get them into school without vaccination.

In regard to vaccination in pregnancy, I have vaccinated a very large number of cases in pregnancy, believing that the danger was trifling, and I have never seen a single instance where it proved of the slightest harm. I have had the misfortune to see more than 50 cases of death from small-pox in pregnant women. The most that you would fear from vaccination would be the loss of the child. That would almost always result if the mother contracted small-pox, and she herself is almost certain to die. Now, then, you have your choice between the chances of small-pox, in which you may well expect to lose both mother and child, and the chances of vaccinia, in which you have little reason to fear any bad result. It seems to me that the question reduces itself to a very plain conclusion.

Dr. R. H. Ferguson, of Dorchester: Dr. Grinnell, of the University of Vermont, told me of an instance in which a cadaver in the dissecting-room was found to have died of small-pox, and quite a number of students had been examining it and had been going about amongst people attending to their duties. When it was found to be so, he isolated as many of the students as he could get hold of, although many of them had gone home. He telegraphed to the demonstrator of anatomy who was in Washington at the time, and received reply saying that it was utterly impossible for

a person to get small-pox from a cadaver with small-pox. Now, I have looked up the literature on the subject, and I have found nothing that would lead me to suppose that a person cannot get small-pox from a cadaver, and would like to hear what Dr. McCollom has to say on the subject.

Dr. McCollom: It is perfectly possible for a person to take small-pox from a cadaver. I think the man who makes the statement that this is impossible is very much mistaken. The mere fact that the students who had been exposed did not have the small-pox was good luck, or they had been very well vaccinated.

Dr. W. T. Clark, of Worcester: In my capacity as health officer I frequently receive notices from physicians who have vaccinated children several times without any effect. I either refer these children back and ask the doctor to try again, or do it myself. I would like to ask Dr. McCollom if he has ever seen a case that he never succeeded in vaccinating.

Dr. McCollom: In answer to Dr. Clark's question, I will say, that I have known of one instance where a person could not be vaccinated. That man was vaccinated fifteen times, but it was found that his mother had had small-pox when he was in utero. I think there are very few cases that cannot be successfully vaccinated if the physician will persist.

Dr. E. F. Gage, of Winthrop: I would like to ask Dr. McCollom how long an attack of small-pox will confer immunity to the disease?

Dr. McCollom: I do not know as I can quite answer that question, but I do think that an efficient vaccination is just as effective. I think where there is an epidemic even if a person had small-pox ten or fifteen years before, it is well to vaccinate,

Dr. Chase: I would like to ask Dr. McCollom if vaccination of a woman in pregnancy would confer any immunity on the child?

Dr. McCollom: I do not think it would.

Dr. G. N. P. MEAD, of Winchester: I should like to ask what is considered a successful vaccination. As we all know, there are some cases where there is some redness, a little swelling, slight pain in axilla, and a very slight discharge. A crust forms, and in a few weeks there is no scar.

Dr. DURGIN: I will say, that, as far as my observation is concerned, in primary cases especially, all the stages of vaccination should be well defined. There should be the well-defined vesicle, pustule, umblication and crust, covering a period of time not less than three weeks. Of course in re-vaccinations we find the appearance of the arm falling short of typical characteristics of vaccination. I regard this as a very important question, and if we have not already wearied Dr. McCollom too much, I would like to hear from him.

Dr. MCCOLLOM: As far as primary vaccination is concerned, I think the appearance is very characteristic. Regarding the secondary vaccination, it is sometimes very difficult to decide whether the vaccination has taken or not, but if the arm begins to show redness and slight irritation at the end of twenty-four hours, the vaccination is not a success. On the other hand, if in a secondary vaccination the patient in six days has a small vesicle surrounded by an areola, you may be sure that person has been successfully revaccinated.

ARTICLES XI.–XIV.

THE USE OF ALCOHOL IN MEDICINE.

THE NUTRITIVE VALUE OF ALCOHOL.

By FRANCIS G. BENEDICT, Ph.D.
OF MIDDLETOWN, CONN.

THE BASIS FOR THE USE OF ALCOHOL IN THERAPEUTICS.

By ARTHUR N. CUSHNY, M.D.
OF ANN ARBOR, MICH.

THE INFLUENCE OF ALCOHOL UPON INFECTION AND ITS USE IN THE TREATMENT OF ACUTE INFECTIOUS DISEASES.

By SAMUEL J. MELTZER, M.D.
OF NEW YORK CITY.

THE COMMON USE OF ALCOHOL AS A BEVERAGE.

By GRAHAM LUSK, Ph.D.
OF NEW YORK CITY.

READ JUNE 11, 1902.

ARTICLE XI.

THE NUTRITIVE VALUE OF ALCOHOL.

By FRANCIS G. BENEDICT, PH.D.
OF MIDDLETOWN, CONN.

In view of the widespread use of beverages containing from 3 to 70% of ethyl alcohol the inquiry as to whether this chemical compound possesses any nutritive value is a proper one. The inquiry is of especial importance to the medical practitioner, since alcohol is used to a very considerable extent in therapeutics and dietetics.

In discussing the nutritive value of any food material it is necessary to see in how far the substance under examination possesses certain chemical and physical properties common to our ordinary foodstuffs, and in how far it performs the commonly accepted functions of food, and what disadvantages, if any, exist as to its permanent and universal use. We may say that a food material should (1) be palatable *per se*, or be capable of being prepared in a palatable manner, otherwise the ingestion of sufficient quantity to play any rôle in nutrition would not long be tolerated; (2) it should be capable of being absorbed as it passes through the alimentary tract; (3) when absorbed it must serve either to furnish tissue-building material and thereby aid the growing organism and repair waste, or it must furnish fuel to keep the body warm and supply energy for internal or external muscular work—in performing either or both of these functions it must of necessity spare body material from con-

sumption; and (4) it should "agree" with people, that is, cause no stomach and other disturbances.

In the fullest and widest sense of the phrase "nutritive value" we should doubtless include an estimate of the cost of such an ingredient of a general diet.

Without considering the question of palatability or pecuniary cost of alcohol in its various forms, we come at once to the question of its absorption in the body.

The absorption of any material through the mucous lining of the alimentary tract is largely dependent on the solubility of the material and the size of the molecule. When we consider the size of the proteid molecule of our common food materials, the complex molecule of the fats and even the relatively large molecule of the soluble carbohydrates, the simple structure and size of the molecule of ethyl hydroxide, leads us to expect what actually does occur, namely, a very rapid and complete absorption. This absorption takes place in the stomach, and indeed is so rapid that comparatively very little alcohol may enter the intestine. We may then properly speak of alcohol as predigested, for the action of digestive juices is not necessary to prepare it for absorption. This is true of but few of our recognized food substances.

While nearly all soluble materials would be absorbed to a greater or less extent in the alimentary tract, only those which contribute directly to the support of the body can properly be considered as possessing any nutritive value, and we should now determine whether alcohol when once absorbed can perform either of the functions of food, that is, build and repair tissue or supply energy for fuel and muscular work, or is excreted unchanged. Formerly the opinion prevailed that alcohol while readily absorbed was excreted unchanged, as is, for example, the coal-tar, sweetening substance, saccharin, and consequently *a priori* could be of no benefit to the body. A most careful examination of all the excretory products has, however, shown that in

doses not exceeding 72.5 gm. per twenty-four hours, less than 2% of the alcohol is excreted as such from the body of a healthy man.* In no possible way can there be any great error in this observation, for the subjects were enclosed in an air-tight respiration chamber, and the amount of alcohol in the ventilating air current, urine and perspiration was determined with great accuracy. We may say that in moderate doses, at least, the alcohol is practically entirely oxidized in the body, but a small portion, in general 2 to 4% being excreted unchanged.

This fact being established the results of the transformation may be more closely studied. The proteid molecule, the chemical basis of muscular tissue, is characterized by containing the chemical element nitrogen in exceedingly complex combination, and it has been found that only that portion of our food containing nitrogen in similar form can contribute to the structure or repair of muscular tissue. Obviously the simple molecule of ethyl hydroxide containing no nitrogen cannot be considered as in any sense performing this important function of food. It must be borne in mind, however, that but a small part of our ordinary diet is capable of performing this function, and we must class with alcohol, as thus failing to build muscular tissue, the carbohydrates and fats of the diet from which the body obtains the larger share of its energy.

If we reason again from the structure of the alcohol molecule, we shall see that it is also incapable of contributing directly to the synthesis of fatty tissue, for from the well known composition of the fats we know that in their synthesis certain complex radicals are necessary, that is, the long carbon chains of the fatty acids or carbohydrates and the residue of the glycerine molecule, a chain of three carbon atoms. All experiments thus far† have failed to de-

* Memoirs of the National Academy of Science, Sixth Memoir, 1902, viii, pp. 392–393.

† Rosenfeld: Der Einfluss des Alkohols auf den Organismus, 1901, p. 31.

monstrate or even imply that alcohol can enter into the structure of the body, and we are led to the belief that not only it cannot build muscular tissue, in which failing it resembles the carbohydrates and fats, but unlike these it cannot also contribute to the structure of the adipose tissue. It is, in a word, in no sense a tissue-builder and therefore fails in one of the most important functions of a food.

We shall see later, however, that even though the organism is probably unable to build tissue from alcohol, alcohol does nevertheless exercise a very powerful influence on the formation and store of fatty tissue.

Physiological research has proven definitely that alcohol is oxidized in the body, and if the law of the conservation of energy obtains in the living organism, as we have every reason to believe it does, the potential energy of the alcohol must be transformed into kinetic energy in the body, either in the form of heat or internal and external muscular work. As the body needs a certain amount of heat to keep it warm, alcohol may thus be of benefit in furnishing a supply of heat.

Although a large number of experiments on the physiological action of alcohol have been made both on man and animals, the effect on the production of heat within the body was not investigated until 1890, when Reichert* made a number of short experiments (five to six hours) on dogs. These experiments, while extremely interesting and carried out with this investigator's usual skill and care, have not the bearing on the question immediately under discussion, namely, its action on man, that we see more perfectly in experiments with men. Of these latter experiments all have been made with the Atwater-Rosa respiration calorimeter at Wesleyan University.† The method of experimenting may be briefly described as follows: The subject is placed on a prescribed diet, suited as nearly as possible to the

* Therapeutic Gazette, February, 1890.

† Bulletin 63, Office of Experiment Stations, U. S. Department of Agriculture, 1899.

needs of his body, and after a four days' preliminary digestion experiment enters the respiration calorimeter. The chamber consists of a tight-walled copper box large enough to permit a man to live with comparative comfort for from three to twelve days. A ventilating current of air furnishes the means for determining the gaseous excretions, while careful analyses of feces and urine and perspiration, as well as food and drink, give the factors for striking a balance of income and outgo of matter. As the energy of the food is measured by combustion with oxygen in a calorimetric bomb,* and the dry matter of the feces and urine similarly burned, the measurements of heat radiated from the body complete the data for striking a balance of income and outgo of energy after making due allowance for the body material gained or lost. The amount of water vapor, carbon dioxide and alcohol† in the air current is determined. The appliances for measuring heat are novel and insure great accuracy. As a direct check on the accuracy of the apparatus for determining water, carbon dioxide and heat, a weighed quantitity of pure ethyl alcohol is burned in a lamp inside the chamber and the products of combustion as well as the heat measured. The amounts as determined agree with the theoretical quantities calculated from the weight of alcohol burned to within 1%—an accuracy sufficient for physiological experimenting.

When the factors for income and outgo are compared in these experiments we have a striking demonstration that the law of the conservation of energy obtains in the living organism, for it is found that the average of all experiments shows an agreement between the potential energy of the material actually oxidized in the body and the kinetic energy liberated in the oxidation in form of heat, which is inside of 1%. In some experiments 72.5 gm. of alcohol, equivalent to about 500 calories of energy, were substituted in a diet

* Storr's (Conn.) Experiment Station Report, 1897, p. 199.
† Journal American Chemical Society, 1898, 20, p. 299.

for fats and carbohydrates. The close agreement observed in all experiments with the non-alcohol diet was obtained in all the experiments with alcohol.

Having thus seen that alcohol in quantities here used, that is, 72.5 gm. per day, is oxidized in the body and the latent energy transformed into heat, it remains to be seen in what way, if any, this liberation of heat is of service to the body. If, as is commonly supposed, the alcohol is rapidly burned in the body and the heat thus liberated dissipated in a short time, the value of such a supply of heat may be questioned; but when we examine the analytical data we see that there was neither an excessively rapid combustion nor was there any abnormal radiation of heat from the body.* The experiments of Reichert, previously referred to, did indeed indicate a slight increase of heat radiation as the result of administering alcohol; with man this was not noticed, though the experiments were frequently of from five to nine days' duration.

It is well known that with excessive doses of alcohol the pharmacodynamic action on the nervous system results in a marked lowering of the body temperature.† The temperature observations made during the experiments with the respiration calorimeter fail to show any diminution in body temperature—a result that is in accord with practically all previous work in which moderate doses of alcohol were ingested. It is reasonable, therefore, to assume that the energy furnished by the alcohol does aid the body in maintaining its temperature and may be of service in the performance of internal and external muscular work. The proof of this latter point is, however, not at hand. That the energy furnished by alcohol does not in part, at least, contribute to the performance of muscular work, involves the assumption that the energy thus furnished is of a different

* Memoirs of the National Academy of Sciences, 1902, Sixth Memoir, viii, 272-276.

† Schaefer's Physiology, 1898, i, 820.

nature from that yielded by the fats and carbohydrates in their combustion. We may say that it is highly probable that such a difference does not exist, and assume with an equal degree of probability that the energy required for muscular work is drawn from the oxidation of alcohol as well as from the other food ingredients. As a matter of fact, when the total energy transformed with a diet containing no alcohol is compared with that transformed in a similar, parallel experiment in which 500 calories of the diet are furnished by alcohol, no noticeable difference appears. So far, then, as these experiments go, and with the quantities of alcohol used, no difference can be detected between the energy furnished the body by alcohol and that furnished by the ordinary nutrients of food, and, though failing to build tissue, alcohol can properly be classed as a source of energy to the body.

In thus furnishing fuel to the body it must spare either other food material or, what is still more important, body material from consumption. A diet deficient in energy results in an immediate draft upon the body material, chiefly fat, for the lacking energy. Alcohol added to such a diet invariably lessens the quantity of body material disintegrated. Similarly a diet just sufficient to maintain the body in equilibrium, when augmented by alcohol, results in a storage of fat in the body, or, if the diet is more than sufficient and fat has been stored, the amount stored will be increased. Marked instances of this protecting power of alcohol for fat are to be found in the published data of the Middletown experiments.

The contention has frequently been made that alcohol, even in moderate doses, influences the general metabolism of the body in such a way as either to increase or diminish the production of carbon dioxide and the consumption of oxygen. Widely varying results have been obtained on this point, though the best and most trustworthy work

shows that the variations in the carbon dioxide production and oxygen consumption are no larger than the variations appearing in a diet containing our ordinary nutrients.

Another and more serious question has been the effect of alcohol on proteid metabolism. Does the ingestion of a moderate amount of alcohol in twenty-four hours result in a more rapid disintegration of muscular tissue? This question naturally arose from the observation frequently made that in many instances the day after the ingestion of alcohol following an alcohol-free diet the nitrogen elimination in the urine is noticeably greater than before.* If we carefully examine all the experimental evidence it must be admitted that on the whole no tendency of alcohol to exert a direct poisonous action on proteid metabolism can be noted, for while the experiments of short duration in most instances show a somewhat greater elimination of nitrogen in the urine on the few days immediately following the ingestion of alcohol, the experiments of long duration invariably show that in a few days the normal excretion is soon re-established, and in many instances a gain of nitrogenous material to the body is clearly shown. The whole matter rests on the loss of a gram or two of nitrogen; and in interpreting the results of experiments of this nature, it is necessary to take into consideration several important points: (1) The elimination of nitrogen in the urine is not a measure of the tissue destroyed; (2) the diuretic effect† of alcohol might account for the slight increase in nitrogen elimination generally appearing for a few days after the ingestion of alcohol; (3) any marked change in diet, even when the nitrogen content is unaltered, is often accompanied by a marked disturbance of the nitrogenous equilibrium, and whatever fluctuations of the equilibrium are caused by alcohol in the diet are no greater than those

* Rosemann: Der Einfluss des Alkohols auf den Eiweisstoffwechsel, Arch f. d. ges. Physiol, 1901: Bd. 86, pp. 307–503; Rosenfeld: Der Einfluss des Alkohols auf den Organismus (1901) pp. 7–22.

† Rintaro Mori: Arch. f. Hygiene, (1887) vii, 354.

caused by the ingestion of fat* under exactly similar conditions. The whole body seems to undergo a process of readjustment when a new diet is given, and in some instances the first few days are marked by a loss of nitrogen in the urine, as is the case when alcohol or fat are included in the diet, while in others the change is accompanied by a gain of nitrogen to the body.

We may safely say that the most accurate experiments give no positive proof or even valid ground for asserting that alcohol in moderate doses influences proteid metabolism any more than does fat. It cannot properly be designated as "proteid poison,"† but it can and frequently does protect body proteid from consumption.

While when used moderately alcohol may be said to "agree" with most people, its rapid absorption precluding difficulties of digestion; when used in excess, on the other hand, it produces, as do many of our common food materials, stomachic and other disturbances that are frequently of a serious nature. Unlike an inordinate fondness for sweets, nature does not rebel so vigorously against an excessive use of alcohol, and while opsomaniacs are relatively uncommon, dipsomaniacs are only too common. Even the coffee and tea habits cannot be compared in any sense with the strong habit so frequently acquired by users of alcohol. It is just this habit-forming property of alcohol leading to its consumption in excessive quantities, under which conditions the distinctly toxic properties of the compound are so manifest, that brings us to a halt in our estimate of the nutritive value of alcohol. The toxic properties of alcohol are not possessed by any other common food material, and consequently must be considered a drawback to its habitual use for nutriment in quantities large and small. Furthermore, if excessive doses are so disastrously toxic, may we not reasonably question the effect of moderate doses and

*Kayser: v. Noordens Beiträge, H. 2, S. 1.
†Miura: v. Noordens Beiträge, H. 1, S. 1.

suspect an insidious action, that may not be apparent in the necessarily short chemical, physical and physiological examination, of the subject, food and excreta, which is afforded by the usual experimental period. Experiments in the chemical or physiological laboratory, no matter how accurately made, can never of themselves furnish the data for determining satisfactorily the effects of the persistent use of moderate quantities of alcohol upon the general bodily and mental condition of man. It is to the pathologist and psychologist that we must look for answers to this question. While accurate and impartial observations on this point are rarely found, surely the work of Kraepelin,* Aschaffenburg† and Hodge‡ is significant in its indications, and we must, at least, hold that alcohol has in many instances, even in moderate doses, a retarding action on many vital processes, and accordingly should be used for nutriment only with full cognizance of such action.

From a review of the work up to the present time, it can be seen that it would be manifestly unfair not to accord to alcohol a value as fuel and as a protector of body material — a value that may at times be of incalculable service to the physician, but would it not be equally manifestly unfair to deny that the latent toxic property of alcohol is such that in many, if not in the majority of cases in which it is used as a food, such use is irrational?

In health obviously the multitude of other materials whose nutritive value is beyond dispute renders the use of alcohol as a food entirely unnecessary. Pathological conditions in which nutriment from this source alone can be used are comparatively rare, though in many cases, as in acute infectious diseases, doubtless the ease of absorption and high calorific value more than counterbalance the toxic effects. Upon the intelligence of the physician, however, should rest the use of this compound as nutriment.

* Psychologische Arbeiten, vols. 1 and 3.
† Inaugural Dissertation Jena (1894).
‡ Popular Science Monthly, 50, pp. 796–812 (1897).

ARTICLE XII.

THE BASIS FOR THE USE OF ALCOHOL IN THERAPEUTICS.

By ARTHUR B. CUSHNY, M.D.

OF ANN ARBOR, MICH.

I HAVE, first of all, to thank you for the compliment you have paid me in inviting me to take part in this discussion. It is true that I might have selected another topic in preference to one which has been in the past and still is the cause of so much heart-burning as the therapeutic use of alcohol. But, at the same time, the position of the pharmacologists has been so much misunderstood, that I am very glad to have an opportunity to place it in its proper light. And as a pharmacologist I first must utter a protest against the prevalent idea that we base our statements wholly upon animal experiments. Any observation as to the action of a drug that is made in such a definite way that it can be verified by others is gratefully accepted by us, whether the subject of the experiment be a human being, sick or well, or one of the lower animals. But where a physician gives merely an impression, colored by his temperament or his hopes, we cannot take cognizance of it; and in this the pharmacologist differs in no whit from the medical profession, in which the danger of accepting subjective impressions as evidence is generally recognized.

In regard to the use of alcohol in therapeutics, perhaps more than in the case of most drugs, this tendency to substitute impressions for verifiable facts prevails, and in look-

ing over the more recent therapeutic literature of the subject I have been struck by the almost complete absence of observations in which changes in the pulse, respiration or temperature are recorded in any more definite way than by such expressions as "improved, strengthened, relieved." The experimental pharmacology of alcohol is now fairly well known; may we workers in the laboratory not expect something more definite from the clinicians?

The difference between the experimental observers and the clinicians in regard to alcohol is largely one of interpretation and not of facts. Each has exceeded the limits of the province in which alone his opinions should be paramount and has invaded that of the other, in which his experience is limited. Thus the experimentalist, basing his opinions on a limited number of observations, may be heard announcing that alcohol is of no value in disease. He forgets that effects that are deleterious in one organ are often more than counterbalanced by changes wrought elsewhere, and that our knowledge of the action of drugs and of pathology is far too limited to permit us to deduce general rules in therapeutics. On the other hand, the clinician lays himself open to attack when he attempts to interpret in scientific terms the phenomena of which he is a witness at the bedside. As long as he confines himself to stating that alcohol is of value in a disease, he is beyond the reach of a laboratory worker. His statements may be disputed by other clinicians, but the experimentalist is incompetent to do so. But when he explains that the effects he has observed are due to the action of alcohol on the heart or to its stimulating the central nervous system, he invades the territory of the experimental investigator, who has a right to criticise such interpretations on the basis of his own results, which, though few in number, permit of more accurate inferences being drawn from them. The discrepancy between the views of different observers in the question of alcohol is, therefore, to

be dissipated by more mutual consideration; the clinician, while maintaining the correctness of his observed facts, must be prepared to modify his theories in view of the experimental results; the laboratory worker must accept the clinical facts, as equally valid with those in his own experience, and must adapt his theories to these facts or abandon them.

I have thought it necessary to make these prefatory remarks because there seems to be a quite unnecessary divergence of views as to the question of alcohol. I shall now consider successively the changes elicited by alcohol in the central nervous system, the respiration, the heart and the digestion, and the question of its value as a food, and shall attempt to determine how far the experimental results agree with the clinical.

The effects of alcohol on the central nervous system differ very considerably in different individuals. In the lower animals they are marked by depression, the symptoms being exactly similar to those of chloral or of the other narcotics. Occasionally, a transient excitation occurs, but this is due to the irritant action in the stomach, and is absent when more dilute solutions are administered or when the alcohol is ingested in small repeated doses. In human beings the same depressant effect is often seen, the first symptoms being more or less drowsiness and heaviness, which subsequently pass into slumber. But in the majority of cases the first effects elicited are feelings of good-fellowship, well-being and liveliness, with increased confidence in the mental and physical powers. This is often followed by some excitement, marked by loquacity, laughter and exaggerated movements of all kinds. The face is flushed, the eyes bright and the pulse and respiration are accelerated. Larger quantities lead to uncertainty and inco-ordination of the movements, shown in difficult and stammering speech and staggering gait, and still larger amounts induce sleep and narcosis, which may pass into complete anæsthesia, and eventually

prove fatal. The symptoms of intoxication, therefore, appear to differ in the lower animals and in man, for, while in the former there are no signs of excitement, in the latter this feature may be very marked in the beginning. And even in man the effects of alcohol offer marked contrasts in different individuals and even in the same individual at different times. For the excitement is very much more marked when drinking is indulged in in company with others, while when alcohol is taken with less exhilarating environments or in solitude, the excitement stage is very much less marked or may be entirely absent.

The effects of alcohol have been explained in two different ways: The first view, which appears at first sight to be the more natural one, is that it stimulates the cerebral cells to greater activity and then depresses them; that is, alcohol is believed to act on the cerebral gray matter in the same way as strychnin on the spinal cord. And this view, that alcohol is a central nervous stimulant, is still very widely entertained both by the laity and by clinicians, while the majority of experimental observers rather leans to the view suggested by Schmiedeberg and supported by Bunge, Kraepelin and many others, that the stimulation of the brain is only apparent. According to this theory, the excitement is caused, not by the augmented vitality of the nerve cells, but by a loss of the associations which ordinarily retard the expression of mental activity. To adopt a mechanical simile, the brain may be compared to an engine fitted with powerful brakes. An acceleration of the motion may be due either to increased power of the engine (stimulation) or to the brakes being raised, and it may be very difficult for an onlooker to determine which is the true explanation. The apparent evidences of increased mental activity under alcohol, however, have proved for the most part illusory, when carefully investigated. The closer one approaches to the engine, the more evident it becomes that what appeared to be the

result of increased power is really the effect of the removal of the brakes. One argument against the stimulant action of alcohol is the narrow limits to which it is confined. All the central nervous stimulants with which we are acquainted act on some particular part, in small doses, it is true, but when larger amounts are ingested, the stimulant action spreads over a wider area of the central nervous system and gives rise to the symptoms characteristic of stimulation of that area. Caffein, for example, first stimulates the mental functions apparently, but in larger quantities involves the motor sphere of the brain and may finally stimulate the cells of the cord. Alcohol, on the other hand, appears to have only a depressant action on nervous tissues, except in the human cerebrum. It is true that the exceptional development of the human brain might permit of a departure which is without analogy with other forms of poisoning.

But when the excitement stage is more closely investigated, it becomes apparent that all the cerebral functions are not facilitated by alcohol. It is common knowledge that under the influence of alcohol an individual may be more brilliant in conversation, more witty, more social, more generous in sentiment, but that he is not so careful in his statements, and has not that consideration for his own position or that of others which he ordinarily manifests. He loses his self-control and self-restraint, and thereby often proves more entertaining and social than in ordinary life, but he also loses his sense of responsibility and his power of discriminating between the trivial and the important or between the merely plausible and the actually proved. In a word, it is generally recognized that some of the highest functions of the brain are thrown out of action by alcohol administered in quantities which induce the phase of exhilaration. The further question is what functions are actually increased in activity and how far is this increase dependent upon the reduced activity of the processes which are depressed by al-

cohol. The question is of course a psychological one, which can be answered only by a psychological analysis, which is as yet very far from complete. But it is obvious to everyone that the exhilarations may be due to a loosening of the associations which ordinarily control our actions and feelings. The normal adult, in an environment which in a child would cause every symptom of exhilaration, maintains his self-control, partly because the position has lost its novelty, that is, he has associations which are wanting in the child, partly because the exuberance and gesticulations natural to the child are kept in check by his sense of the ridiculousness of these manifestations in later years. Remove these inhibiting associations, destroy the self-control of adult life, and the man becomes a child again in his sense of exhilaration and symptoms of excitement. It would take more time than I have at my disposal to enter further into these considerations. My object is attained if I have suggested to you that the symptoms of intoxication, which are ordinarily regarded as indicating stimulation of the brain, do not necessarily involve this interpretation, but may be explained by a removal of those associations which ordinarily inhibit the external manifestations of emotion.

Much valuable evidence as to the effect of alcohol on mental processes has been gathered in the psychological laboratories, especially by Kraepelin and his pupils, who have subjected the question to a large number of ingenious psychological tests in the past few years. Even before Kraepelin took up the theme, however, it was generally recognized that the supposed effect of alcohol in increasing the ability to do work and increasing the resistance to fatigue was really contrary to practical experience. Over and over again it has been demonstrated beyond question that regiments supplied with alcoholic liquors are less capable of long marches and suffer more from fatigue than others to which no alcohol is issued. But Kraepelin has shown this to hold

good for forms of work in which more mental capacity is required than the marching of soldiers. For typesetters do a smaller amount of work and make a much larger number of misprints when even a small amount of alcohol is allowed, than when they perform their work without this drug. And other tests performed by Kraepelin and his students all show a lessened capacity for mental work and a smaller ability to keep the attention directed to one object. Thus, when a student was set to learn a number of meaningless syllables or a row of figures, he took a longer time to do so and made more errors in repeating them under alcohol than normally. Similarly arithmetical calculations of all kinds were carried out much more slowly and with more errors, and the writing was slower and gave the impression of being that of an uneducated person who seldom had occasion to use his pen. Only in one respect was any increased aptitude shown, namely, in the transformation of an idea into movement. This might suggest that the motor functions are accelerated under alcohol, but another explanation is equally applicable, namely, that an ordinary movement is hampered by a series of associations and that when these are destroyed by alcohol the movement may be carried out more quickly. At the same time the associations in ordinary life not only retard the movement but also restrict and direct it, and the result of their absence under alcohol is shown by the awkwardness and inaccuracy with which the movement is executed.

It is noteworthy that in these experiments, those mental processes which were ordinarily performed readily were less retarded than others in which the subject was less practised and which required more effort. That is, the powers most recently acquired and most readily lost are those on which alcohol first acts, while those operations which have become habitual are less impaired. This is in complete accord with what is observed in the earlier stage of intoxication or exhilaration. The most recent acquisitions in adult life are

the power of self-control and the feeling of self-respect which are manifested in regard for the conventions of life and in the prudence which leads one to avoid many procedures which in earlier life he might have indulged in without reproach. And under alcohol these are the first mental processes to be disordered. *In vino veritas:* in intoxication the natural man is exposed, stripped of the trammels of convention and robbed of the fruits of experience and education.

Many other results of these experiments are of interest, but cannot be entered on here. One feature which bears a familiar aspect and which was brought out clearly in many instances, is the confidence of the subject of the experiment that his efforts were unusually successful under alcohol while the impartial record showed results far below the level attained during abstinence.

In all of these experiments the amount of alcohol taken was small, in no case sufficient to induce any of the more evident symptoms of intoxication. It is therefore of great interest to find in the later publications of Kraepelin's laboratory, that distinct impairment of the psychical powers were found to persist for a much longer time than would be anticipated; the average efficiency was not regained until from twelve to twenty-four hours had elapsed after the alcohol had been taken. If this proves to be true in the case of persons who have acquired a tolerance for alcohol, many individuals are never completely normal, their mental powers never recovering entirely from one dose of alcohol before the next is taken. Some interesting observations regarding the length of time during which alcohol may be retained in the body unoxidized have recently been made by Grehant. He found that in animals to which a large dose had been given, alcohol may be detected in the tissues for twenty-four hours.

The results of these investigations on the mental state under alcohol as well as many others which I have not dis-

cussed, appear to place the theory that alcohol acts as a narcotic upon a firm basis. I do not hold that it is absolutely determined, but some more satisfactory evidence must be brought forward in favor of the stimulation theory before it can be considered as a rival of that of Schmiedeberg. And this evidence must be more definite than is ordinarily offered. It must define what functions are stimulated, and must not be a repetition of the old statement, that the tone and vigor of the nervous system is maintained by alcohol.

But the acceptation of the narcotic theory is not equivalent to condemning the therapeutic use of alcohol for its effects on the brain, as is apparently supposed. We must rather consider whether the results aimed at by the clinician, when he prescribes alcohol, have not been misnamed stimulation, and whether they are not narcotic in nature, and hence in entire agreement with the experimental results. And it is difficult to understand what use we could make of alcohol in disease were it a cerebral stimulant; for no mental exertion is encouraged in acute disease, but every effort is made to restrain the activities of the patient in every direction. The room is darkened, every noise is excluded as far as possible, exciting conversation and reading are forbidden. How absurd it would be to take all these precautions to induce bodily and mental rest, and at the same time to order a drug with the intention of promoting cerebral activity. The truth appears to be that when the clinician applies the term stimulant to alcohol, he uses the word in quite a different sense from that in which it is understood by the experimental observer. His meaning is less definite, and he wishes to indicate only the improvement often noted in the general condition without considering whether this is due to an augmentation or a retardation of the mental processes. The same confusion is met with in regard to opium, which also was formerly regarded as possessing stimulant properties which recommended its

use in acute diseases accompanied by cerebral symptoms. The result of both drugs is in reality cerebral depression, which manifests itself in a condition of euphoria. The disease often loses its strongest ally when the anxiety and worry of the patient are allayed by alcohol. He feels less concern and more resignation, not through his soul rising triumphant through the stimulant action of alcohol, but through his brain being less capable of dwelling on his disabilities, because it is partially narcotized.

The depressant or narcotic action of alcohol, then, far from being in conflict with its clinical use, supplies a definite and logical explanation of the improvement so often noted at the bedside after its administration. And this effect of alcohol in allaying the subjective symptoms, relieving the nervous strain, and promoting the rest and comfort of the patient, is not surpassed by that of any other drug. For while the narcotics are legion and new ones arise monthly to supply the place of those discredited, few of them act on the cerebrum with equal power without involving other organs. None of them can be taken more readily and act less on the alimentary tract, and the great majority of them reduce the activity of the vital centres in the medulla along with that of the higher brain. The distinctive feature of alcoholic action is a slight depression of the mental faculties leading to euphoria, which can be elicited without any other organ being affected detrimentally. This action is not a specific one, and in this era of specifics may not appear to be of first-class importance; but as long as we have no specific for many common disorders, anything which induces rest, and thus allows the reparative processes a fairer field, must be considered in combating disease.

The action of alcohol on the respiration has long been a subject of experimental investigation, and although the main facts are now agreed upon, their interpretation is still a matter of dispute. During the exhilaration and excite-

ment of intoxication the number of the respirations is generally found to be increased, but this may be due to the muscular movements and need not indicate any augmentation of the activity of the respiratory centre, and as the excitement is not elicited in therapeutics these observations do not concern us. A number of estimations have been made in cases in which no excitement was induced, and though the results show a good many discrepancies, the evidence thus gained is to the effect that the amount of air inhaled is generally increased by alcohol even when no signs of excitement are displayed, and in some cases when a well-marked narcotic action is elicited.

This is not indisputable evidence, that the respiratory centre is stimulated, however, for some increase in the respiration is induced reflexly by food of any kind when it passes into the stomach, and alcohol may be expected to induce more powerful reflex movements from its irritant action in the stomach. Some investigators have, therefore, ascribed the changes in the respiration after alcohol to reflexes arising from its local action in the stomach, and have denied that it has any direct effect on the respiratory centre except a depressing one, which is only elicited by enormous doses. The question is certainly a difficult one to determine. In some experiments which I have performed the alcohol was diluted and disguised by the addition of syrup containing some volatile oil. I found that a considerable increase in the air breathed occurred after the administration of the mixture, but in some cases in which the same amount of syrup was given without alcohol a similar augmentation followed. Some years ago Loewy attempted to measure the excitability of the respiratory centre more directly than by merely measuring the amount of air breathed before and after alcohol. Unfortunately his experiments were too few to permit of a positive inference being drawn, but they certainly failed to indicate any

increase in the irritability of the centre. It is impossible to take up a definite position in regard to this question, then, and we must wait for further research to determine whether the augmentation of the air inhaled under alcohol is due to direct central action or to reflexes arising from the stomach. It may appear that the whole matter is one of academic interest only, without practical bearing on therapeutics; but on further consideration this is seen not to be the case. For, if the augmented respiration is caused only by the local action in the stomach, this indicates that much of the surplus oxygen is used in doing the work of absorbing the alcohol, and that the rest of the body profits to a correspondingly small extent from the increased aeration. And further, in those conditions in which a respiratory stimulant is indicated, the centre must be conceived to be in such a depressed state that it fails to react to the stimuli supplied by the gases of the blood. This has to be combated by increasing the irritability of the centre, for it would seem improbable that nerve cells which fail to react to the powerful natural stimulus supplied by the accumulation of gases in the blood, can be aroused by the weaker ones arising from gastric irritation. The use of alcohol as a respiratory stimulant is, therefore, not supported by experimental investigation, but on the other hand it is not wholly condemned. When we turn to clinical experience for enlightenment, we find an equally divided verdict. Some physicians advise alcohol in respiratory failure, while others have found it useless, and it is impossible to determine which view is correct, for neither side gives any measurements.

While the position of alcohol as a respiratory stimulant is thus still unsettled, no question whatever can be entertained as regards its effects on the circulation. In small doses it does not affect the heart except through the increased movement when excitement is induced, while in

very large amounts it acts as a cardiac depressant. An ounce of whiskey injected into a dog's vein has the same effect on the heart as the inhalation of chloroform for a short time. Smaller doses have a correspondingly less deleterious action, but whatever dose is used, no increase in the efficiency of the organ follows. Examinations of the pulse in the human subject give a similar result; no change in the rate can be made out apart from the insignificant acceleration due to the act of swallowing and the local irritation; and no alteration has been found in the area of cardiac dulness or by the various methods of estimating the blood pressure in man. As regards the blood vessels, alcohol causes some dilation of the superficial vessels; but this has no effect on the general blood pressure in animals. Very large quantities reduce the blood pressure to some extent, and smaller amounts injected intravenously have the same effect.

Yet alcohol has long enjoyed a reputation as a cardiac stimulant. This is due in part, I think, to the fact that it is often given in desperate cases, and if a favorable result ensues, some credit is given the treatment, while if the case proves fatal it is ascribed to the disease. But I do not think it is to be discarded in circulatory disorders, for here again its cerebral action may prove of value by lessening the anxiety and other mental symptoms. Similarly morphin is of value in hemorrhage, not because it acts on the circulation or accelerates coagulation, but because it prevents restlessness and puts the body in the most favorable position for repair to take place. It may seem paradoxical, but the real basis for the use of alcohol in acute circulatory disturbance appears to be the fact that in ordinary quantities it has no action on the circulation, while it promotes rest.

The effects of alcohol on digestion have been the subject of much controversy, but I think it may be held as proved

that any beneficial results are due to changes effected in the walls of the stomach and intestines, and not to alcohol influencing favorably the activity of the ferments, at any rate in the forms in which it is ordinarily taken. The slight irritant action on the gastric mucous membrane certainly entitles alcohol to a position in the somewhat indefinite group of carminatives, although it has not yet been shown to increase the gastric movements. Its presence in the stomach and intestines appears to promote the absorption from these organs. But apart from these properties which alcohol possesses, in common with many other drugs, it has been shown to alter the gastric functions in a way which, so far as is known at present, is unique. After alcohol reaches the blood a very profuse secretion of fluid takes place into the stomach, it matters not whether the alcohol is given by the mouth or injected as an enema. This fluid sometimes contains considerable quantities of pepsin, sometimes merely traces, this apparently indicating that alcohol does not actually augment the secretion of ferment, but only increases the fluid which carries with it what pepsin is preformed in the gland. It has been recognized of recent years that one of the functions of the stomach is to dilute the food in order to permit of the action of the ferments, and this action of alcohol in promoting the escape of fluid into the stomach may be of advantage in cases in which the mucous membrane fails to react to the presence of food. Again, it has been demonstrated beyond question that the secretion of the stomach depends largely upon the appetite and the taste and odor of the food. Persons to whose senses alcohol and its accompanying ethers are grateful, may thus be able to digest food taken with alcohol better than they otherwise would, and some supporters of the narcotic action of alcohol believe that its effect in promoting sociability and "making glad the heart of man" may also predispose the individual to enjoy his food, and thus indirectly accelerate its digestion.

It is obvious that these various factors may reinforce each other in some conditions, while in others they may have less obvious results, or may even be in abeyance, and this is in accord with the results of clinical observations. For while some have found that in the human stomach alcohol accelerates proteolysis, others state that this process is retarded, and a third group holds that it is first retarded but afterward accelerated. The truth probably is that the effects of alcohol vary in different individuals and in different conditions of the mucous membrane of the stomach, and further investigation of the subject is required before the indications for its use as a stomachic can be defined accurately. In all probability far more cases of gastric disorder are treated with alcohol than are really benefited by it, for it is to be remembered that the preparations of the bitter tonics which are most in vogue contain a high percentage of alcohol.

The question of the nutritive value of alcohol, which has occupied such a large place in the public mind recently, has been discussed by Dr. Benedict, a much more competent authority than I. I desire only to draw your attention to one or two points bearing on its usefulness as a food in diseased conditions. The first of these is that alcohol must be used in large quantities in order to be of any value as a food, for half a pint of whiskey is equivalent to some two ounces of fat. This amount of whiskey is, however, sufficient to cause marked cerebral symptoms, and the use of alcohol as a food in ordinary circumstances may therefore be dismissed from consideration. But in some cases, notably in high fever, it is found that much larger quantities can be borne without any symptoms of cerebral disturbance, and at the same time large draughts are being made on the resources of the body, and it is impossible to replenish these by ordinary means, for the fats and carbohydrates are not assimilated. Heroic measures are required in order to

maintain the integrity of the tissues, and alcohol may be taken into consideration as a readily available source of energy, even if not devoid of suspicion in ordinary circumstances. And it is a matter of common observation that a quantity of alcohol equivalent to the fats of the food in health induces no symptoms of intoxication in these cases, so that there would seem to be no reason to object to it as a nerve poison. But objections have been raised from another source, since it has been shown repeatedly that large quantities of alcohol predispose to infection, and some extremists have gone so far as to state that the recent results of Abbott, Laitinen and others condemn the use of alcohol in any dose and under any circumstances.

I do not wish to enter upon the division of the subject that has been assigned to my friend, Dr. Meltzer, but I should like to state that in all my experiments in which alcohol administered during the disease proved deleterious, the animals showed more or less distinct symptoms of intoxication, while in the cases with which I am dealing at present the patients show an extraordinary tolerance. I desire to speak guardedly on this question, but think that it is safe to state that while the food value of the small quantities of alcohol ordinarily tolerated is insignificant, in certain cases it is borne in quantities which materially aid in maintaining the resources of the body, and that there is no convincing evidence that when used for a short time in those cases it lessens the resistance to the invasion of disease.

As regards the use of alcohol as a narcotic, Laitinen's results with small quantities of alcohol (equivalent to 2 to 4 oz. of whiskey per day for an average man) given after infection do not indicate any distinct deleterious action except in those experiments in which animals were inoculated with tuberculosis, in which, as he states, it is impossible to measure the dose accurately. Besides, it is to be noted that the narcotic effects of alcohol could scarcely have the same

beneficial results in animals as are daily observed in patients, for the subjective effects of disease are entirely absent or only slightly developed in the former. It may be added that anæsthesia in any form reduces the resistance to infection also, and that if we are to exclude alcohol for this reason, we must logically abandon chloroform, ether and opium.

A danger in the therapeutic use of alcohol, which is generally recognized, is that of forming the habit. No one can deny that if this is of frequent occurrence, it is of itself sufficient to condemn all use of alcohol. But cases of chronic alcoholism arising from its medicinal use are fortunately rare and form an insignificant fraction of those arising from its social use. I can only wish that the medical fraternity were as guiltless in regard to other drug habits, such as that of morphin. There are conditions in which the prescription of alcohol must be guarded on this account,—notably in chronic diseases and as a stomachic,—but the most serious feature in regard to the use of alcohol, is that it is not sufficiently appreciated that it is a drug and ought to be used as one. At present it is too often advised merely as a matter of routine, very often at the suggestion of the patient or his friends, without consideration as to whether it is rationally indicated or whether experience has proved it of value in the conditions under treatment. I am strongly of opinion that this haphazard use of alcohol explains in large part the skepticism in regard to its value, and that if half as much consideration were given to its suitability to the condition as is given before most other drugs are prescribed, the position of alcohol in therapeutics would be more assured than it is, and a great gain would accrue from the more accurate definition of its sphere of usefulness.

In conclusion, I would sum up these remarks by saying that alcohol deserves a place in therapeutics as a narcotic, and to a less extent as a stomachic, and in certain conditions as a food. I would not disguise the fact that it has drawbacks,

like every other drug, but would plead that we must not allow ourselves to be guided entirely by consideration of any one of its properties, but must estimate in each case whether the sum total of its effects, good and evil, will benefit the condition.

ARTICLE XIII.

THE INFLUENCE OF ALCOHOL UPON INFECTION AND ITS USE IN THE TREATMENT OF ACUTE INFECTIOUS DISEASES.

By SAMUEL J. MELTZER, M.D.
OF NEW YORK CITY.

It is a very old idea that wine is a good thing for the sick and the infirm. "Drink no longer water, but use a little wine for thy stomach's sake and thine often infirmities," wrote a wise and holy man nearly nineteen hundred years ago. And a few centuries before him wine was ordered for patients by the greatest physician history knows to record. Through nearly twenty-four centuries, from Hippocrates down to our own days, physicians of all creeds and systems have employed alcoholic beverages as an aid in their struggle against diseases. Systems of medicine, however, influenced at all times the extent to which this therapeutic agent was employed; and as in the long history of the science of medicine numerous systems came and went, rose and fell, continual changes, rises and falls also mark the history of the employment of alcohol as a medicine. The influence of the systems, however, extended only to the degree, the extent to which alcohol was used; beyond and independent of any system there seems to have lived, and still lives in the minds of all observant practitioners a strong empirical conviction that some dose of alcohol "does the patient good." The task of each new theory was to provide the philosophers among

the physicians with arguments why alcohol should be given and how it affects the patient or the disease. The influence of the theory upon the practice seems to vary with the mental attitudes of the different civilized races: the more philosophical the mind of a nation is, the greater is the influence of a theory, for good or for evil. The so-called Brunonian system offers an instructive illustration. John Brown was an Englishman who lived in the eighteenth century. Influenced probably by a conception of irritability, as it was established by Haller, Brown brought forward the theory that life is kept up by stimuli, and diseases are either asthenic or sthenic; their cure, therefore, has to be either an increase or a decrease of stimulation. The employment of alcohol in sickness found in this theory the most promising explanation: alcohol is a stimulant. In Germany this theory was received with enthusiasm, and it had the unfortunate effect that physicians administered alcohol to their patients in very large, harmful doses. This abuse, says vonJaksch,[1] killed hundreds of patients. With the downfall of this theory a strong and long-lasting reaction set in against the abuse, as well as against the proper use of alcohol in diseases, and Binz[2] tells us that in the forties of the last century a physician had to defend himself publicly for giving moderate doses of alcohol to a typhoid patient.

In England, the home of Brown,—but also the home of the Baconian method of studying natural science by actual observation,—the Brunonian theory was never received with great enthusiasm, and exercised only a moderate influence upon the practice of using alcohol as a drug. And here the downfall of the theory did not produce such a reaction against the legitimate use of alcohol. On the contrary, already in the thirties and forties of the last century we see clinicians like Stokes, Graves, and especially Todd, exerting their influence to enlarge the indications for the employment of alcohol. It was the inauguration of that long period

during which in England alcohol was quite extensively used in febrile disease, and which is now apparently on its slow decline.

In the second half of the last century, with the advent of the experimental method in studying the sciences of medicine, alcohol became the subject of extensive experimental investigations, and it was especially in Germany that numerous experimental studies were published by Binz and his pupils and by many other prominent investigators, on the relations of alcohol to the temperature, the metabolism of the animal organism, the circulation, respiration, etc. Greatly through the influence of these scientific investigations alcohol again became a factor in therapeutics in that country. In the eighties and nineties of the last century we again find in Germany a greater enthusiasm for the use of alcohol in typhoid fever, pneumonia, puerperal fever, etc., than we meet with at the same period in England. Binz[3] summarized the reasons for giving alcohol in febrile diseases in the following manner: "Alcohol is an antipyretic, is a convenient food, and it stimulates the circulation and respiration." In addition to these points the recent progress in medicine brought out a new factor in the discussion of alcohol as a medicine.

Since the eighties of the last century most of the diseases for which alcohol was prescribed received a new character: they are now known as infectious diseases, and the question arose, what relation does alcohol have to infection? Clinicians like Runge and others, who are enthusiastic supporters of the use of alcohol in septic diseases, expressed their belief that alcohol strengthens the body in its struggle against the virulent invaders. Binz[4] says that "the opinion of our clinical teachers may in general terms be expressed as follows: Alcohol in large doses increases in an extraordinary manner the power of resistance to septic poison." Is this clinical opinion borne out by experiment?

Gentlemen, one of the theses with which I was entrusted to report to you to-day is on that subject, and reads as follows:

THE INFLUENCE OF ALCOHOL UPON INFECTION.

A consideration of the effects of its use upon immunity and upon the resistance capacity of the individual to septic infection.

EVIDENCE OF ANTITOXIC ACTION, IF ANY.

It is therefore the first task of my paper to offer a report on the experimental work done so far on the subjects in question.

Binz, in fortifying his above-mentioned opinion, quotes one experiment of his own. He injected into two dogs septic material taken from a decubitus of a typhoid patient. One dog received 10 cc. of alcohol three times within seventeen hours; this dog survived, the other dog died. Binz mentions this experiment in several places, but it does not appear that he made more than one experiment.

In attempting to produce cholera in animals, Koch tried first to introduce alcohol into their stomachs. This method was employed systematically by Doyen,[5] and he found that animals which received alcohol became indeed infected with cholera, while controls remained free. Doyen was inclined to believe that it was due to some local effect of the alcohol upon the acidity of the stomach.

Thomas[6] has given rabbits for two consecutive days about 5 cc. per kg. alcohol diluted four times with water, and then injected cholera cultures intravenously. Five alcoholized animals died, the control animals withstood infections with much larger doses. In one other experiment Thomas found that the serum of an alcoholized rabbit lost its bactericidal effect upon cholera organisms.

The first extensive series of experiments were made in this country by Dr. Abbott[7] of Philadelphia at the instigation of the Committee of Fifty on the Alcohol Question. Rabbits

were given alcohol 5 to 6 cc. per kg. per day before or after inoculation with either the streptococcus pyogenes, bacillus coli, or the staphylococcus pyogenes aureus. In the inoculations with streptococcus nearly all the alcoholized animals died, while most of the non-alcoholized survived. In the inoculations with the colon bacillus the alcoholized animals also showed a diminished resistance to the infection, although not so pronounced as in the inoculations with streptococcus. The inoculations with the staphylococcus did not bring out clear results.

Deléarde[8] has studied in rabbits the effect of alcohol upon immunization against rabies, tetanus, and anthrax. His conclusions are that in rabies the administration of alcohol after infection will destroy the required immunization, and that in tetanus and anthrax no immunization will take effect, if alcohol is given simultaneously. (Deléarde's statements make the impression of his having had only one experiment for each of his conclusions.)

Laitinen[9] studied the influence of alcohol upon the susceptibility to infectious material on 342 animals—dogs, rabbits, mice, guinea-pigs, chickens and pigeons. As infectious material he used anthrax, tubercle bacilli and diphtheria toxin. Alcohol was given either for a number of days previous to the infection, or started soon after. The quantity of alcohol used in these series of experiments was not less than 5 cc. per kg. per day; in many instances the animals were distinctly intoxicated. The anthrax experiments demonstrated unmistakably the deleterious effect of the alcohol. Most of the alcoholized animals died, most of the non-alcoholized animals survived the infection. The experiments with tubercle bacilli gave less striking results: In one series the alcoholized animals died after an average of thirty days, the non-alcoholized animals after thirty-four days, and in a second series the alcoholized animals died after an average of twenty and the non-alcoholized after an average of twenty-

two days. In the experiments with the diphtheria toxin the alcoholization hastened the death of inoculated animals. According to Laitinen the blood of the alcoholized animals seemed to show a decrease in its alkalinity; this suggestion was already thrown out by Abbott. (In a later publication,[10] the original of which was not accessible to me, Laitinen has studied the effect of smaller doses of alcohol. Of seventy-five alcoholized animals 61% died, and of seventy non-alcoholized animals 50% succumbed to the infection.) Laitinen credits Valagussa and Ranelleti[11] with the statement that alcoholized animals show a diminished susceptibility to the toxin of diphtheria.

Pawlowsky[12] has given whiskey to guinea-pigs for some time, and found that their resistance to infection with staphylococcus citreus was considerably diminished. He also states that the alcoholized animals reacted to a subcutaneous injection with a diffuse œdema, while in non-alcoholized animals an injection caused an abscess. (I would suggest here that this difference might perhaps have its explanation in the statement of Massart and Bordet,[13] that alcohol, even in great dilution. is negatively chemotactic.)

Gruber[14] and Koegler have administered to guinea-pigs small doses of alcohol (.1 to 1.5 cc. per kg.), and then infected them with the pneumobacillus of Friedlænder. They found that the alcoholization increased in general the mortality of the animals.

Goldberg[15] studied the influence of alcohol upon the natural immunity of pigeons to anthrax Of twelve pigeons which were inoculated with a certain amount of an agar culture of anthrax, all survived; of fifteen other pigeons which were inoculated with the same amount of anthrax, but which received soon after the inoculation, or a day or two later, comparatively large doses of alcohol, twelve died. The same occurred in another series in which alcohol was administered for some time previous to inoculation. In a third

series eighteen pigeons were inoculated with fatal doses of anthrax and one-half of this number received twice a day ½ cc. of alcohol. From the results in this series Goldberg concludes that the administration of alcohol did not prolong the life of the pigeons. (But it should be added that the results also demonstrated that the alcohol did not shorten the life of the animals.)

Ausems,[13] working under Talma, also found that the administration of alcohol in small doses to rabbits before infection, diminishes their resistance to different infections. Small doses of alcohol given after the infection reduce the temperature but do not influence the course or the severity of the infection. Large doses of alcohol reduce the temperature but increase the length and severity of the disease.

In contrast to this long list of experiments adverse to alcohol, I am able to record only one brief statement in favor of alcohol: according to Gruber,[14] an intraperitoneal injection into guinea-pigs of a freshly sterilized culture of prodigiosus, causes a strong collapse of the animal; the temperature sinks to 34° C., and death follows in a few hours. Now Gruber states in a provisional brief report, that the frequent administration of small doses of alcohol to such animals prolonged their life for many hours, and two animals were even restored to health.

Such is the report on the influence of alcohol upon infection. It unequivocally reads adverse to alcohol. Of the ten different systematic investigations none came out in favor of alcohol. It was studied on dogs, rabbits, guinea-pigs, mice, chickens and pigeons. The investigations extended to infections with cholera-vibrios, streptococcus, colon bacillus, anthrax, tetanus and tubercle bacillus, rabies and the toxin of diphtheria. In all these cases it was found that alcoholization before infection increases the susceptibility; alcoholization after infection decreases strikingly the resistance of the animal; it also destroys the natural and

the artificial immunity, and is rather favorable than antagonistic to the fatal influence of the toxin of diphtheria.

What do these experiments on animals teach us regarding the influence of alcohol upon infectious diseases in human beings?

Gentlemen, I venture to say that these experiments — striking as they may appear — teach us as yet very little new, and nearly nothing definite, and for the following reasons: In every one of these experiments in which the alcoholization proved to be definitely unfavorable, the smallest dose which was administered to the animals amounted to 5 cc. of absolute alcohol per kg. animal, and in many cases the dose was a good deal larger; for instance, 10 cc., and even more. For rabbits, the animal which was extensively employed, the fatal dose is — according to Dujardin & Baer[17] — 7.5 cc. per kg. Let us take the minimum dose of 5 cc. as a basis, and compound it for a human being of a medium weight of 75 per kg.: it would amount to 375 cc. absolute alcohol, or more than a pint and a half of strong brandy. Now who has ever given that quantity in a single dose, or even in two doses, to a patient? Surely this would lead in a normal individual to the most acute alcoholism, and perhaps even to a fatal issue. We would certainly expect, without any experimental evidence, that any infection in addition to such a toxic dose of alcohol will prove fatal. Furthermore, we also knew long ago that alcoholic individuals are less resistant to the effect of infectious diseases.

On the other hand, we have abundant proofs that similar, and even larger, doses of alcohol, distributed over twenty-four hours, administered to human beings down with infectious diseases, do positively not show such strikingly deleterious effects as were observed in animals. Abbott or Laitinen had in their respective experiments series of four or five alcoholized infected animals, all or the most of which

died, while the controls survived. Now Runge[18] published a table of twenty grave cases of puerperal fever with a recovery of 80%; all of these patients received for days and weeks very large doses of alcohol. If this does not prove anything else, it proves at least conclusively that large doses of alcohol are not as harmful to septic human beings as they seem to be to septic animals.

Observations of a similar kind have been made time and time again in typhoid fever. Between 1884 and 1887 I followed the custom of that time and treated typhoid cases with large doses of alcohol. I remember having given to a young girl of about 30 kg., nearly one pint of whiskey, every day for eleven consecutive days, without any untoward symptom. And it was during this period that I did not lose a single case of typhoid fever. Not that I wish to put up the claim that the alcohol treatment was the cause of the recoveries; I quote this experience to show that large doses of alcohol did not lower the resistance of my patients to their infection. Hundreds of other physicians had similar experiences. These experiences are reliable clinical evidences, sufficient to warrant the assertion that doses which proved so harmful to animals, are comparatively harmless when administered to human patients in a proper medical manner. These clinical observations seem to me to be well-established facts, equal in their reliability to those derived from the animal experimentation. I shall try, however, to offer some explanations for the differences in the results.

In the first place, as already indicated above, in nearly all the experiments the large quantities of alcohol were administered in one or two doses. In administering alcohol to patients we distribute the daily quantity in six or even twelve doses. By a single, large dose the body cells might become damaged beyond recovery. Then there are some other significant points. Take, for instance, the mode of administration: in most of the experiments the

alcohol was introduced into the stomach of the animal through a stomach tube. Abbott states that this method led in many cases to erosions and inflammation of the stomach, which in some cases resulted in distinctly complicating infections. Another significant fact is that in none of the experiments mention is made as to how the animals were fed; it is possible that the alcoholized animals had a good deal less appetite for food, and ate less than the control animals. We notice in many protocols the remark of the loss of weight of the alcoholized animals. The physician, as you all know, pays a great deal of attention to the feeding of his patient.

Furthermore, the reduction of the amount of alcohol per kilogram animal does not offer a basis for a fair comparison. The resistance of large animals is greater than that which is due to them on account of their number of kilograms. An animal of 75 kg. may resist a quantity of poison seventy-five times the dose which is fatal to an animal of 1 kg.

Moreover, as we know, even individuals of the same species vary considerably in their resistance to the effects of alcohol. There is surely a great difference in this regard between different species, and a per kilogram dose which is fatal to the pigeon might indeed be of no account to a man.

Finally, I would call attention to one fundamental difference between the experimental infections of animals and infectious diseases in man,—a difference which might have an important bearing upon the different results. In the experiments the infection or intoxication is brought on at once by a single injection of a nearly fatal dose of bacteria or toxins. The organism is taken by surprise before it could bring its defensive forces into action against the enemy. On the other hand, a disease is rather similar to a break within an immunization experiment. That main stage which we term disease is preceded by a slow invasion of bacteria or

their products before and during the long incubation time. When the patient and his doctor recognize the existence of an infectious disease, the struggle between the human organism and its virulent enemy is already well on the way for some time, and there are already at the disposal of the attacked body—besides the well-organized multitude of normal defensive forces—a good many newly formed immunization products, capable of taking care of a good many of the battle-worn invaders. Alcohol, therefore, might prove more harmful to the already greatly exhausted enemy in diseases than to the fresh, unrestricted virulent intruders in experimental infections.

However all this may be, we are bound to state that the large number of experiments on animals at present on record can at the utmost serve only as a warning to the practitioner to keep away from too large doses of alcohol in infectious diseases. They do not throw any light upon the question whether conservative doses of alcohol properly given to a patient sick with an infectious disease, and under proper management and care, are harmful, indifferent or useful. New experiments are required, with a proper consideration for the actual conditions existing in the infectious diseases of man.

I come now to the second thesis assigned to me for discussion. It reads as follows:

THE USE OF ALCOHOL IN THE TREATMENT OF THE ACUTE INFECTIOUS DISEASES, ESPECIALLY IN TYPHOID FEVER AND PNEUMONIA.

An explanation of the results obtained by its use in the light of our pharmacological knowledge of the drug.

Gentlemen, the use of alcohol in medicine is now on the defensive; it is again apparently passing through one of its historic crises. There are many causes for the present decline. Alcohol as a drug shares partly the fate of drugs in general, which are less prescribed at the present time in in-

fectious diseases than in previous years. The advent of serums also contributed to the decline in the use of alcohol. In diphtheria, for instance, antitoxin has greatly reduced the former extensive use of alcohol in this disease. Then the national and international movements against the abuse, as well as the general use, of alcohol in health has, no doubt, a good deal to do with much of the activity displayed against its employment in sickness. The most spirited attacks, however, which the use of alcohol as a medicine had to face have been made within the last year or two, and are based apparently on purely scientific grounds. The conclusions to which these writers arrive are simply sweeping, and read as follows:[19]

The bacteriological experiments, say the scientific opponents of alcohol, have demonstrated that alcohol favors infection. Alcohol is a proteid poison and not a food, proclaims Kassowitz, and some others agree with him. The newer school of pharmacology has proven, they say, that alcohol is not a stimulant but a depressant. Furthermore, alcohol is not a stomachic, is of no use as an antipyretic, and is a poor narcotic. In short, there is no earthly reason for its use in disease, and there are weighty reasons against its use there.

Permit me to discuss the validity of some of these arguments. Beginning with the question of the influence of alcohol upon infection, and the evidence to be derived from the existing bacteriological experiments, I shall refer to what I have already stated above, namely, these experiments as they stand now cannot be used as an argument against the employment of alcohol in infectious diseases in man, and we need not discuss this point over again.

The next important point in the discussion on the value of alcohol as a medicine is its supposed stimulating quality. It is chiefly for this effect that many physicians prescribe alcohol for their patients. The opponents of alcohol refer

us, however, to Schmiedeberg and his school, who hold that alcohol does not stimulate anything, but, on the contrary, exercises exclusively a depressing, paralyzing effect upon the functions of the body. It depresses the higher nerve centres, by which a degree of narcosis and a lack of concern is produced, and it causes paralysis of the vasomotor centres; in short, alcohol does not increase, but lowers, the vitality throughout the animal body. The so-called stimulating effect is simply a misinterpretation of the phenomena alcohol produces.

Gentlemen, in full appreciation of the high merits of Schmiedeberg and his learned followers, I venture to state that their theory suffers from some incompleteness in their analysis of the phenomena of stimulation and paralysis. We can all agree upon the proposition that stimulation has always the effect of increasing the display of a normal function, whatever this function may be; and that depression or paralysis invariably lowers or abolishes the normal display of a function. However, if the term "stimulation" has in pharmacology and medicine the same meaning which is attached to it in physiology, I cannot see why alcohol should not act as a stimulus. Any kind of a solution which is not isotonic with the body fluids will act as a stimulus on irritable tissue. Even cocain or other local anæsthetic will cause at first a stimulation of the tissues with which it comes in contact. The kind of an effect which such a stimulus might produce depends, of course, upon the nature of the tissue which it irritates. Application of alcohol to a motor nerve will cause contraction of a muscle; application to the peripheral end of the vagus will cause slowing of the heart beat; application to the chorda tympani will cause dilatation of the blood vessels of the submaxillary gland. Now, the slowing of the heart by stimulation of the vagus appears as a reduction of activity, the effect of the alcohol here is nevertheless not a depression, but a stimulation of a function, because it sets to work the inhibitory mechanism of the heart.

Now it is with regard to just this point that Schmiedeberg's theory shows a deficiency. This theory has in view only either an increase of motor or sensory activity, or a decrease of vitality, but it offers no room for stimulation of an inhibitory mechanism by which a certain activity might appear diminished and the vitality at the same time being actually increased. Let us take, for instance, the flushing and turgescence of the skin after the use of some dose of alcohol. Schmiedeberg considers it due to a paralysis of the vasomotor centre. I believe I will not meet with any contradiction from physiologists when I say that the flushing of the skin is due to either a stimulation of the vasodilators, or an inhibition of the tonus of the vasomotor centre; in either case it is the effect of stimulation of a normal function, and not a parlysis of a function.

It must be mentioned that Binz, too, who believes in the stimulating effect of alcohol, has in mind apparently only the motor phenomenon. He therefore insists that alcohol increases the number of heart beats under the supposition that a slowing of the heart would not be the effect of a stimulation. A practitioner would hardly appreciate the advantage of a stimulation which would increase the heart beats of a typhoid patient from 120 to 140 per minute. The observations of vonJaksch,[20] according to which alcohol reduces the pulse in fever patient, are surely more to the point: alcohol stimulates the inhibitory mechanism of the heart, and thereby reduces the number of its beats.

Before continuing the discussion on the relation of alcohol to inhibition, I wish to make a few remarks on the relations of alcohol to vasoconstriction and vasodilatation. The opponents of alcohol deny its stimulating effect upon the circulation, among others on the grounds that alcohol either causes a lowering of the blood pressure, or has no effect upon it; it surely does not increase the blood pressure. Permit me to say that actually there are very few reliable investi-

gations on the influence of alcohol upon the conditions of the blood vessels. The statement which is now frequently made without qualification, that the blood pressure is lowered by alcohol, actually refers only to ante mortem conditions in fatal doses. The writings of the opponents of alcohol are full of peculiar quotations. Paessler, for instance, is often quoted as saying that alcohol does not influence the blood pressure; what, however, he actually said, is that his method dose not permit the drawing of any conclusion regarding the effect of alcohol upon blood pressure.[21] Kobert[22] worked with isolated organs, that is with organs removed from all vasomotor influences; he is nevertheless quoted in support of the assertion that alcohol does not influence normal blood pressure. Although a further analysis of some of the writings might prove indeed interesting reading, I shall not indulge in it, but shall confine myself to the simple remark that to my knowledge there is nothing in the literature on the subject which can seriously disqualify the following considerations:

It is an established fact that after taking a moderate dose of alcohol the skin becomes turgescent and flushed. We have seen above that this is due to vasodilatation in the skin. This vasodilatation could be either local, confined to the skin only, or it could be a part of the general dilatation of all the blood vessels in the body. In this latter case there would be a lowering of the general blood pressure. For a decision of this alternative I would like to call attention to the following facts: The area which is controlled by the splanchnic nerves contains the largest part of the body's blood. When this area is dilated it withdraws a good deal of blood from all the peripheral organs. When this occurs the skin is cool, pale, cyanotic and sallow. When the skin is red and turgescent we can with safety assume that there is no general vasodilatation, no "bleeding into the vessels of the splanchnic area"; the vasodilatation in the skin is therefore only local.

But as the quantity of the body's blood remains constant, each local vasodilatation has to be compensated by a constriction of the blood vessels in another region. We know that there is a mutual nervous arrangement between the peripheral organs and the splanchnic area to such an effect that each vasodilatation of the peripheral organs is accompanied by a proportionate vasoconstriction in the splanchnic area. The administration of a dose of alcohol, which has the visible effect of causing redness and turgescence of the skin, has the actual effect of distributing the blood in such a manner as to cause vasodilatation in the peripheral organs, including the brain, and vasoconstriction in the splanchnic area. It is obvious that under these circumstances the blood pressure measured in the carotid artery, or in any other manner, will not be changed and will not indicate that a change in the distribution of the blood has taken place.

I maintain, therefore, that alcohol in not too large doses does not affect the general blood pressure, but effects some change in the distribution of the blood, which may be indifferent, or even harmful to a normal person, but which can prove to be of great importance to a patient with an infectious disease. We know through the investigations of Romberg and Paessler[23] that infections, with the pneumococcus, for instance, cause a general vasodilatation of central origin. The patient has then a "bleeding in the splanchnic area" and blood is withdrawn from all the peripheral organs: the skin, the extremities, the central nervous system, the heart, the kidneys, etc. Such a patient might have a high rectal temperature, but has cold extremities, livid skin, small pulse, mental depression (from cerebral anæmia), and voids very little concentrated urine. We give him alcohol and cause a degree of vasoconstriction in the splanchnic area and a vasodilatation in all the peripheral organs, with the result that the heart receives more blood and the pulse gets fuller, the extremities warmer, the skin gets red, becomes turges-

cent, dissipates more heat, thereby reducing the temperature; the patient's mind becomes brighter, and his urine is increased and less concentrated. In short, the patient is stimulated and feels better. This effect is surely a stimulation even in the restricted sense of the term in which it is used in the schools of Binz and Schmiedeberg. In other words, alcohol does not affect the circulation by increasing or decreasing the general blood pressure, but by instituting such a distribution of the blood which is especially favorable to patients sick with infectious diseases.

We return now to our discussion of the relations of alcohol to inhibitory mechanisms. The contention of Schmiedeberg and his school, that a great many of the symptoms caused by alcohol appear to be of a paralytic nature, is perfectly correct. The climax of them is the deep narcosis in acute alcoholism, which looks indeed like paralysis. There is, however, a cardinal difference between the temporary states of narcosis and states of paralysis which usually involve a permanent loss of some degree of vitality. And it seems proper to avoid the confusion of the two thoroughly different states. Whether narcosis could be looked upon as a state of inhibition is a suggestion I do not care to discuss here. However this may be, it seems to me to be quite certain that some of the lesser effects of alcohol upon the nervous system can be interpreted as inhibitory phenomena. We know that certain conditions of temperature or of electricity, or certain degrees of stimulation, or certain substances favor either excitation or inhibition. I should not attempt to discuss here this intricate subject, but shall state my hypothesis in a few words; it is this: Alcohol favors the irritability of the inhibitory mechanisms. I do not doubt in the least that alcohol stimulates also motor or sensory nerves, or the organs of activity in general. I only suggest that in a certain dilution and at a certain stage, alcohol, when in contact with the nerve cells, either directly stimulates preferably some of

the normal inhibitory mechanisms of the body or increases their irritability. As a consequence of this stimulation the perception for finer differences becomes decreased, the reaction time is prolonged, the formation of associations and the irradiation of stimuli within the central nervous system become restricted. Also the tonicity of all the muscles is reduced and the execution of motion is rendered difficult. While such changes are surely undesirable in health, they are very beneficial and desirable in sickness. The fever-burned, infected, septic patient with his restlessness, hypersensitiveness, turning and twisting in bed, soreness over all his body, subsultus, etc., is surely greatly benefited when by the administration of alcohol his muscles become relaxed, his sensitiveness diminished and his useless mental activity is restricted. Alcohol enlists the aid of the inhibitory mechanism to procure rest for the toxin-hunted patient.

Strychnin is a stimulant, and alcohol is a stimulant; but strychnin stimulates preferably the motor or active mechanisms, while alcohol stimulates preferably the inhibitory mechanisms. Too large a dose of strychnin causes convulsions, but too large a dose of alcohol causes what looks like paralysis. It is obvious that strychnin, camphor or caffein cannot supplant alcohol; but they might profitably supplement one another.

My defence for the use of alcohol in infectious disease consists in that (1) alcohol stimulates the patient by effecting a favorable distribution of the blood within his sick body; (2) alcohol protects the patient against harmful excitation by stimulating some of the normal inhibitory mechanism of the body.

As to the claim of the opponents of alcohol that it is a poison and not a food, I need not discuss it, as this point has been already well taken care of by an authority more competent to speak on that subject than I. All workers in that line of investigation seem to agree now more and more that al-

cohol, when taken for a longer period, proves to be in the end as good a proteid saver, as fat and carbohydrates are. Ott[24] reported very recently that this proposition holds good also for the administration of alcohol during a febrile disease. Believing as we do that alcohol is a very useful drug we should accept its additional quality as a food with special appreciation. Even if we restrict the administration of alcohol to such small doses as one cc. per kilo in twenty-four hours, it would present the value of 500 calories for an individual of average weight, which is about one-sixth of his daily need. Surely an addition not to be despised.

Alcohol as an antipyretic or as a stomachic we need not discuss here. They are at all events minor qualities, and will not, I believe, materially influence our decision one way or another regarding the use of alcohol.

Some of the radical opponents of alcohol admit its usefulness in states of collapse, but ascribe it to its narcotic effect. In collapse, they say, the patient is greatly alarmed by his condition, and the narcotic effect of the alcohol relieves him of his anxiety. But such effect could be better and more quickly accomplished by a whiff of chloroform:—would the advocates of this theory risk such a procedure? Patients in profound collapse have a vacant look in their eyes and hardly know what is going on around them. After being revived by some alcohol a look of recognition returns into their eyes, and they wish to know what happened to them. Can this be interpreted by assuming that the narcosis brought consciousness to the patient? Gruber's[25] guinea-pigs rallied from their profound collapse, brought on by the injection of prodigiosus cultures, by the administration of alcohol. Did the low temperature of the animals rise again because the narcotic effect relieved them of their anxiety?

But admitting even that the entire usefulness of alcohol rests upon its narcotic quality, as long as it is useful, why not use it? Here is the difference between opinion, based

upon "the light of pharmacological knowledge" and convictions based chiefly upon personal observation at the bedside. The attitude of Sir Samuel Wilkes[26] is an instructive instance. He professes his belief that alcohol is not a stimulant but a sedative, but advocates nevertheless its use in typhoid fever with arguments of the following character. He tells of a girl who was in the hospital suffering from a most severe attack of typhoid; she grew worse until her end seemed approaching. When apparently dying, brandy was administered, and this was continued for two days and nights until an enormous amount had been given. She then began to rally and shortly recovered. Now as regards the effects of this brandy upon her, "I can see," says Wilkes, "only three modes of reasoning: It did her good, it did her harm, it was valueless. Now I think few persons would say that giving two bottles of brandy in a few hours was of no importance, that its action on the system was *nil* and might be disregarded. It might then be said that it did harm, but in such a case as this there was only one step more to death, and this must have inevitably ensued had the patient got harm. The only alternative left is that it did good."

It is a noteworthy fact that among the opponents of alcohol there is not a single clinical man of note who came out unqualifiedly against the use of alcohol based simply upon unfavorable clinical results gathered at the bedside. On the contrary, clinicians ranking high in the profession and thoroughly familiar with the pharmacology of the drug are unwilling to be deprived of its aid in dealing with diseases like typhoid or pneumonia. Curschman,[27] writing on typhoid in Nothnagel's "Handbook," says that in spite of all theoretical objections alcohol is indispensable for the patient, and certain stages of the disease can not be successfully treated without it. He says further that the stimulating effect of alcohol upon circulation and respiration is a fact es-

tablished beyond all doubt, though it may be difficult to find its theoretical explanation. Pel,[28] in his well-known careful address on the treatment of pneumonia, says he is well aware that the theoretical explanation of the effects of alcohol is insufficient, but the experience at the bedside has the last word in this matter, and here we gain the conviction that alcohol stimulates and strengthens the patient. Juergensen, vonJaksch and many other celebrated clinicians express similar views.

Personally I am willing to be responsible for the following two statements: Alcohol in health is often a curse; alcohol in disease is mostly a blessing.

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ARTICLE XIV.

THE COMMON USE OF ALCOHOL AS A BEVERAGE.

By GRAHAM LUSK, PH.D.
OF NEW YORK CITY.

WINE has been used from the earliest dawn of history. Among the Hebrews its production is first attributed to the patriarch Noah, while among the Greeks and the Egyptians it was said to have been introduced by the gods, by Dionysis in Greece and by Osiris in Egypt. Among the older nationalities the vine and the olive were looked upon as the symbols of a cultured civilization. A semi-nomadic people could raise a corn crop, but a settled life and the highest husbandry were required of "him that planteth a vineyard."*

All races of men who can obtain alcohol use it as a beverage in one form or another. If alcoholic beverages be introduced among a nation unaccustomed to their use, much drunkenness follows. G. A. Reid, in a recent book on "Alcoholism," endeavors to show that the races long accustomed to the use of alcohol have lost through early extermination those individuals whose tendency was to drink themselves to death, and who have not, therefore, bequeathed this tendency to their offspring. Thus, a certain immunity is acquired. Scientific temperance would, therefore, mean free rum. This may be partially true, but Herbert Spencer has called our attention to the fact that drunkenness was very prevalent in England a hundred and fifty years ago,

* Encyclopedia Brittanica.

and that great changes have been wrought through an altered public opinion.

There are as many contradictory statements regarding the use of alcohol as a beverage as can be found in the literature of any one subject. On the one hand we find vonVoit* saying the flavor of wine occasionally taken is the noblest, delighting the human sense, and though it be not the milk of old age, still it makes the old feel young. On the other hand, Bunge rails at those who would partake of the excreta of yeast. Hutchinson† quotes Thackery's reported saying that he had got some of his best thoughts "when driving home from dinner with his skin full of wine," whereas Helmholtz, at the Commers given in honor of his seventieth birthday, declared that he had never had a great thought come to him after taking a glass of wine. Strümpell‡ declares that moderate drinking has a cumulative toxic action similar to lead poisoning.

The use of alcohol as a food has been known for a long time. But the general use of alcoholic beverages depends upon two specific properties: (1) Alcoholic drinks possess flavor, and (2) they act upon the central nervous system. A single glass of wine containing 10 cc. of alcohol may render a meal more palatable and bring about a certain amount of what is generally supposed to be central stimulation. In the absence of this flavor and stimulant, others are sought, as in tea and coffee, for example. Before the general use of tea, it is said that Queen Anne made her breakfasts of "beef steaks and small beer."

Alcoholic beverages are numerous and well known, and the custom of their use is varied.

A clear example of the use of beer as a flavor is found among the mountains of the Bavarian Tyrol. Here, excellent cheese is eaten with coarse bread, and the monotony of

* Hermann's Handbuch, 1881, No. 1, vol. vi, p. 430.
† Food and Dietetics, 1901, p. 327.
‡ Ueber die Alkoholfrage, 1898.

this highly nutritious combination of foodstuffs is varied with the light refreshing Bavarian beer. We can appreciate the motto which runs on the monkish brewery: "God bless the beer of Ammergau." It is interesting to note that where wine instead of beer is drunk the cooking also improves. With beer coarse food tastes well, but throughout the Rheinland the delicate *bouquet* of the wines compels a more careful attention to the flavor of the accompanying food.

The brilliant researches of Pawlow* have given us a clearer idea of the action of a flavor than we have heretofore possessed. Many will remember that Bidder and Schmidt found that the mere sight of food by a hungry dog resulted in the flow of gastric juice through his fistula. Pawlow has gone further in this direction. He has prepared a dog with both an œsophageal and gastric fistula. If such dogs be fed with meat or milk or bread, this "fictitious meal" passes out the opening in the neck, and five minutes after the beginning of such feeding a copious flow of gastric juice commences in the stomach. Pawlow was able to demonstrate that the pathway of the efferent impulses to the stomach was through the vagi. After cutting the vagi, this reflex secretion did not take place. Pawlow sought to find if the sensory stimulus for this apparent reflex flow of the gastric juice consisted simply in a local stimulus of the various sensory nerves of the mouth. To this end he prepared a dog with an œsophageal and a gastric fistula and washed the dog's mouth with such substances as hydrochloric acid, pepper, mustard and salt, or the dog was given stones to swallow, all with no avail. Not a drop of gastric juice flowed from the stomach. The dog could be made to swallow stones for hours at a time, passing them out of the fistulous opening of the œsophagus, but this mechanical stimulation had not the slightest reflex effect on the gastric glands. A piece of meat fed at any time during this experiment produces the flow of gastric

* Die Arbeit der Verdauungsdrüsen, 1898.

juice in almost exactly five minutes. This first secretion of gastric juice depends, then, upon the sense of the appetite, and, therefore, Pawlow describes it the "psychical secretion."

The physician in ordinary practice is too apt to think that whatever disagrees with him must of necessity disagree with his patients, forgetting thereby that what may be tasteless or repellant to him may be highly appetizing for them. Whatever stimulates the sense of appetite will aid in the preliminary "psychical secretion" of the gastric juice, and Pawlow himself describes his personal experience after an illness which left him apparently normal except that he was without appetite for his food, all substances tasting very much alike. When in this condition, he found that a glass of wine passing through his mouth, œsophagus and stomach produced sensations resulting in appetite. Wine would seem, therefore, the strongest stimulus to appetite and consequently the strongest stimulus to the flow of the psychical secretion of gastric juice.

Following the psychical flow of gastric juice, the further flow is brought about by chemical reagents, acting upon the neurosecretory apparatus. If egg albumin or bread be introduced into the stomach through the gastric fistula of a sleeping dog, there will be no secretion of gastric juice, for these substances do not contain the chemical bodies necessary for the stimulation of the gastric nerves, and mere mechanical irritation is always ineffective. On the other hand, meat or Liebig's extract of beef similarly introduced will slowly produce a flow, because meat contains substances which stimulate the neurosecreting mechanism. So also pepper, mustard, the amara, and alcohol* directly increase the flow of the gastric juice when introduced into the stomach. Rubner† states that he has noticed that alcohol helps the digestion when meat and fat are fed together.

* Brandl: Zeitschr. f. Biologie, 1892, Bd. xxix, p. 277.
† von Leyden's Handbuch der Ernährungstherapie, 1897, Bd. i, p. 108.

Pawlow explains this by showing that the quantity and enzymatic power of gastric juice produced by the fat meat diet is less than by meat alone. Hence game, which is comparatively free from fat, is given to invalids.

But alcohol seems to have a yet wider influence upon the gastric glands, in that it causes them to secrete when introduced into the small intestine,* or even into the rectum.† No other substance is known which will cause this effect.

We see clearly from the above that alcohol may act very generally throughout the whole intestinal canal, to stimulate the flow of the gastric juice.

The next question which arises is, does alcohol stimulate the flow of the pancreatic juice? Pawlow has told us that the great stimulus to the flow of pancreatic juice is the action of the hydrochloric acid of the gastric juice upon the wall of the duodenum. Hence we see that alcohol which promotes the gastric flow indirectly promotes the pancreatic flow as well.

Pawlow has compared the appetite to a lighted match starting a fire. The psychical secretion of gastric juice begins. Digestive products are formed which stimulate the stomach wall, directly causing a further flow of gastric juice, and finally the pancreatic juice is stimulated to flow by the presence of hydrochloric acid. White of egg introduced into the stomach directly will not be digested. White of egg eaten with appetite may be digested. Chittenden, Mendel and Jackson have shown that alcohol promotes the flow of saliva.

So throughout the intestinal canal alcohol favors the flow of the digestive juices.

It should be added that if too much alcohol be taken there is too great an irritation to the mucous membrane of the stomach, with increased activity and resulting dyspepsia.

* Chittenden, Mendel and Jackson: American Journal of Physiology, 1898, i, 164.

† Radzikowski: Pflüger's Archiv., 1901, Bd. lxxxiv, p. 513.

Also alcohol in the stomach at first prevents the gelatinization of proteid necessary for peptic digestion, but this factor lasts for a short time only, as the absorption of alcohol even from the stomach is rapid and complete.

There is little doubt that alcoholic beverages taken at meal-time promote digestion through the addition of a flavor. It is, however, very doubtful whether in normal persons continued excessive encouragement of the appetite is either necessary or desirable.

Besides giving flavor to the food, alcoholic beverages also act upon the central nervous system. Rubner says that could all men rest when tired, such stimulants as tea, coffee and alcohol might be less used. Two theories have been advanced to explain the action of alcohol on the central nervous system. The first is that alcohol stimulates and afterwards depresses the nerve centres. The second theory, championed by Schmiedeberg, is that alcohol always acts as a depressant, but that the beginning of the depression is first upon the inhibitory nerve centres, thereby leaving the motor apparatus freer to act. There is a diminution of the sense of fatigue, of attention, judgment and reflective thought, and a raising of the self-esteem. Alcohol brings cheer to the tired and to the weak. This factor should not be overlooked in its use in medicine.

The effect of alcohol in physical fatigue is a many-sided one. In considering this subject it must be remembered that there is a peripheral apparatus, the muscle, governed by neurones whose cell bodies lie in the anterior horn of the cord. The discharge of these cell bodies is in turn influenced (1) from the brain by both motor and inhibitory impulses; (2) from the sensory nerves within and near the muscle itself, and (3) from all kinds of sensory nerves from all parts of the body.

It is a widespread belief that fatigue is due to the exhaustion of nerve cells. This theory has been critically

reviewed by my assistant, Dr. R. S. Woodworth.* Dr. Woodworth shows that peripheral sensations, such as soreness, for example, are factors which naturally produce a feeling of fatigue and a consequent tendency to stop work, although in reality the nerve cells and the muscles are fully capable of continuing to act. If the narcotic effect of alcohol dulls these sensations of fatigue, work would seem easier.

It is known that many sensory stimuli entering the cord rouse the motor cells to increased irritability. Thus each sound of the rhythmical beat of a drum stimulates the cord cells that they may more readily discharge. In the same way Féré claims that the sensory stimulation of merely washing the mouth with brandy enables one to do more work.

One can only surmise concerning the direct effect of alcohol on the motor area of the brain or on the cord cells themselves.

Concerning the action of alcohol upon the muscle substance, important results have been communicated to me by my friend, Dr. F. S. Lee of Columbia University, and I am permitted to lay them before you here. Dr. Lee finds that in the curarized frog's muscle, where all the nervous influence is removed, the immediate effect of alcohol is to increase the amount of work done even by 100%. The individual muscle contraction is quicker and fatigue is delayed. The secondary effect, that is, the result after the action of alcohol has lasted for an hour, is directly the reverse of the above, there is a depression, a decreased ability to work. If it were possible to apply these experiments to protoplasm in general and to the nerve cells in particular, we would perhaps say that the action of alcohol was first to stimulate and then to depress.

It seems hardly necessary to refer to the fact that the use of alcohol in armies as a means of obtaining additional expenditure of physical force has long since been abandoned.

* New York University Bulletin of Medical Sciences, 1901, i, 133.

After taking alcohol the sum total of work done will not be increased, but rather diminished. The depressant effect is the predominating factor in the long run.

Where muscular work becomes of the minute, painstaking kind, the action of alcohol is not favorable for its accomplishment. Thus Aschaffenburg* found in the case of four typesetters that the effect of drinking 200 cc. of Greek wine containing 18% of alcohol was to reduce the speed of typesetting on an average of 15.2%, while it was noticed that the accuracy of typesetting was unaffected. vonVintschgau and Dietl† have measured the reaction time before and after drinking two bottles of Rhine wine. The reaction time was much delayed, although the persons thought that they were acting quicker than before.

Concerning the action of alcohol on intellectual activity, Smith‡ has shown that 40 to 80 gm. of alcohol will depress the mental power, as indicated by the ability to memorize or to calculate, and it especially depresses the power to improve by practice.

Fürer§ says that a mild intoxication results in intellectual depression lasting for twenty-four hours or more.

Without doubt alcohol reduces the power to do both physical and mental work. It reduces the power to learn. All the mental work done in the above experiments was of the minute, painstaking sort. Dr. Woodworth has suggested to me that intellectual activity with some swing and freedom about it and aiming only at broad effects might be differently influenced, as seems indeed to be the case with some orators, musicians, and possibly writers.

On the whole, the moderate use of alcohol as a beverage gives additional flavor to the food, and acts as a mild narcotic, often preceded by a period of exhilaration.

* Kræpelin's Psychologische Arbeiten, 1896, i. 608.
† Pflügers Archiv., 1877, Bd. xvi, p. 316.
‡ Archiv. f. Psych., 1895, Bd. xxvii, p. 958.
§ Archiv. f. Psych., 1895, Bd. xxvii, p. 970.

ARTICLES XV.-XIX.

HEAD INJURIES.

NON-OPERATIVE HEAD INJURIES.

By THOMAS J. ROBINSON, M.D.
OF TAUNTON.

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By EDWIN W. DWIGHT, M.D.
OF BOSTON.

INDICATIONS FOR OPERATION ON HEAD INJURIES.

By WILLIAM N. BULLARD, M.D.
OF BOSTON.

GUNSHOT WOUNDS OF THE HEAD AND CRANIUM.

By LOUIS A. LAGARDE,
MAJOR AND SURGEON U. S. ARMY.

TECHNIQUE AND AFTER-CARE OF OPERATIONS UPON THE CRANIUM.

By CHARLES L. SCUDDER, M.D.
OF BOSTON.

READ JUNE 10, 1902.

ARTICLE XV.

NON-OPERATIVE HEAD INJURIES.

By THOMAS J. ROBINSON, M.D.,
OF TAUNTON.

First, just what is meant by non-operative? The term as used here simply means injuries in which no operation on the skull or its contents is advisable.

These cases include injuries to the scalp; certain injuries to the skull; certain injuries to the brain; very often a combination of all three.

Classified, they come under the following heads: Injuries to the scalp; certain forms of vertex fractures; fractures of the base; cerebral concussion and contusion; certain cases of compression; certain cases of laceration.

INJURIES TO THE SCALP.

The only question of diagnosis in contusion of the scalp is in reference to fractures. Effusion of blood beneath the aponeurosis may often be extensive, and sometimes the margin of the effusion may have such a raised, hard feel that it is difficult to rule out a depressed fracture. Here, when the diagnosis is doubtful, it is best to make an open incision.

These injuries are treated simply by cold applications and a suitable compression bandage. If there is a large hematoma which absorbs very slowly time will be saved by making an incision and turning out the clot.

The diagnosis of scalp wounds is of course obvious. Here in addition to the general canons of wound treatment

there are certain special points to be observed, namely, careful hemostasis; the strictest antisepsis; removal of all bruised tissues or those whose vitality is at all questionable, and finally the use of drainage if there is any chance for retention of fluids.

NON-OPERATIVE FRACTURES.

Of vertex fractures simple linear fractures only can be considered in this class; all comminuted, depressed or compound fractures call for operation.

The diagnosis of simple linear fracture is not always easy, especially where there is no wound of the soft parts. Often the point of injury is covered by a thick hematoma through which very little can be felt. Here it is often well to incise the parts and feel the exact condition; this should always be done if there is reason to suspect a depression.

Where there is an open wound the parts can be inspected and the diagnosis is simple. With the eye and finger search carefully for fissures or depressions; do not mistake sutures or Wormian bones for fractures. The important thing is to allow no depressed fracture to pass for a linear fracture, as the whole treatment of the case depends on this point.

The treatment of these cases is simple and expectant: Due attention to any injuries of the soft parts; rest, cold applications to the head, laxatives and diuretics if necessary, and care in the general nursing. If subsequent to the injury signs of compression develop, they are probably due to meningeal hemorrhage, and call for immediate operation.

FRACTURES OF THE BASE.

The vast majority of fractures of the base of the skull come in the category of non-operative cases, this chiefly from anatomical reasons. The base of the skull, except in certain special places, is not accessible to operation. A

class of cases to be excepted from this rule, however, are those of fracture of the anterior fossa; these cases, concerning chiefly the orbital plate, are treated on the same lines as vertex fractures, and will not be considered here.

The diagnosis of basal fractures rests upon three cardinal signs: (1) The appearance of ecchymoses beneath the skin or mucous membranes, due to spread of blood from the seat of fracture; (2) the escape of blood, serum or brain substance from the cavities of the skull; (3) disturbance of function along particular cranial nerves.

Regarding ecchymoses these may be early or late, beneath the skin, the conjunctivæ or the covering of the pharynx. The location corresponds to the region of fracture; ecchymosis of the eye points to the anterior fossa, of the mastoid to the middle or posterior fossa, of the cervical region to the posterior fossa. This last appears only after a few days.

As regards the escape of blood from the skull, bleeding from the ear is the most common; here be sure that the blood comes from the inner ear and not from some wound of the meatus or tympanum. Profuse hemorrhage comes only from fracture.

Escape of serum usually occurs after hemorrhage, though it may begin at once. A free and continuous discharge of serum from the ear, beginning soon after injury, is pathognomonic of basal fracture extending through the petrous bone into the subarachnoid spaces; sometimes when the tympanum is not ruptured the fluid, instead of escaping by the ear, runs down the Eustachian tube into the pharynx.

The escape of brain substance is rare and indicative of extensive and fatal injury.

In addition to these special symptoms there will be in almost all cases brain symptoms of contusion or compression in varying degree, all of which are to be considered in determining the diagnosis.

The treatment is expectant and symptomatic. As the

cases are generally the result of extreme violence and are complicated with serious brain lesions, treatment is often of no avail, and all injured parts should be cleaned as thoroughly as possible; the ear, if injured, should be wiped out and kept plugged with sterile cotton; these means to prevent infection of the external parts and its subsequent spread into the brain: complete rest; cold to the head; extreme care in feeding and regarding bed-sores; attention to bowels and bladder. If later in the case symptoms of compression from hemorrhage or pus appear, operation may be required, provided it is likely that the cause of the compression can be located and removed.

CEREBRAL CONCUSSION AND CONTUSION.

In considering the diagnosis of these conditions, the history of the case is important with reference to the question of injury. If, after an injury to the head, we have quickly loss of consciousness, partial or complete, pallor, disturbances of vision and hearing, loss of muscular power, weak pulse and respiration, perhaps vomiting, we have a typical picture of what is called concussion, a condition which is perhaps best regarded as one of brain shock. The mild cases of concussion differ from the severe simply in degree, the effects of the former being quickly over, those of latter more lasting.

If the condition of cerebral shock just described is persistent, the coma profound and lasting, the pupils dilated and insensible to light, or, if, with reaction of the pulse and respiration, evidences of muscular paralysis or spasm appear, the case is not one of concussion but of contusion. Cases of concussion and contusion shade into each other and the only means of differentiation is the severity and persistence of the symptoms.

In cases without history the chief difficulty in diagnosis is in differentiating between head injuries and cases of coma due to other causes, namely, opium, alcohol, apoplexy, uræmia and diabetes.

In opium poisoning the contracted pupils and very slow respiration will usually mark the case. More difficult is the separation from alcoholism, because head injuries occur so often when persons have been drinking. Absence of alcoholic smell is of positive value, its presence not. In alcoholism the coma is not quite so deep, and temperature is usually subnormal, whereas in severe central cases the coma is absolute, the temperature elevated.

In apoplexy there is as a rule evidence of paralysis either of face or extremities, also a normal or subnormal temperature.

In uræmia there is usually œdema of some part of the body and the urine shows characteristic changes.

In diabetic coma the urine will tell the tale. Therefore, examine the urine carefully in all these cases when it is possible. Consider all the symptoms carefully, and if in doubt wait; time will clear the matter up. Especially, beware of mistaking a cerebral lesion for alcoholism.

TREATMENT OF CONCUSSION AND CONTUSION.

For concussion the treatment is simply that of shock. Rest in bed with the head low; absolute quiet; heat to the body and extremities; stimulants, best strychnin subcutaneously; attention to scalp wounds, if necessary.

For contusion the foregoing; also cold applications to the head. Particular attention should be paid to the excretions for the prevention of auto-intoxication. In cases where swallowing is difficult great care should be used in feeding, as then cases are very prone to pulmonary complications; also, the greatest pains should be taken to prevent bed sores. After the acute symptoms have subsided, if persistent mental and motor changes remain, iodide of potass is said to be of service.

CEREBRAL COMPRESSION.

In many cases of cerebral compression it is very difficult to draw the line between the operative and the non-operative

cases. The general rule, "Operate if it is probable that you can find and remove the compressing cause," is simple in theory but complicated in practice.

Traumatic compression may be produced in several ways: (1) By extravasation of blood; (2) by depressed fractures; (3) by œdema from circulatory changes. The symtoms of general compression are the same whatever the cause. These are mental disturbances; headache; muscular disturbances; then stupor, deepening into profound coma, slow pulse and respiration, pupils dilated and sometimes unequal; elevation of temperature; choked disc; sometimes convulsions. The symptoms of local pressure are various paralyses; hemiplegia, monoplegia or paralyses of different muscle groups, according to the location of the compressing cause.

CASES OF COMPRESSION FROM HEMORRHAGE.

The majority of these cases are due to rupture of the middle meningeal artery or its branches; these are operative and do not concern us here.

On the other hand, most cases of cortical or subdural hemorrhage are non-operative unless the clot is very large where the indications are the same as in meningeal hemorrhage. These cortical cases commonly show a mild grade of pressure symptoms; some mental uncertainty, sleepiness, and slowing of the pulse; often headache and loss of memory.

Compression from depressed fracture is operative. Pressure from œdema of the brain is almost always non-operative. Here the diagnosis is generally conjectural and the symptoms those of general compression without localizing signs.

Aside from operation, treatment of these cases is general and on the lines already laid down.

In cerebral œdema venesection may be of benefit by relieving the venous circulation; also cold to the head to contract the arteries; free purgation and diuresis to promote absorption and excretion.

CEREBRAL LACERATIONS.

Lacerations of the brain substance are usually the result of extreme violence and generally complicated by other factors, such as depressed fractures or hemorrhage. The symptoms are not characteristic, being practically those of contusion, or if the laceration is accompanied by hemorrhage we may have the symptoms of compression. It is often impossible at the time of injury to differentiate between cases of laceration and contusion. Fortunately, the diagnosis is of little practical importance. Later, the case may develop paralysis or contracture of various muscle groups; also albuminuria or glycosuria, bulbar paralysis or various disturbances of the special senses. Results such as these following brain injury would point to a laceration.

The only rule for separating the operative from the non-operative is here the same as in other cerebral injuries; if you can locate the lesion and help it, operate; otherwise do not. In other words, without localizing symptoms, treat the case expectantly.

Treatment is the same as for contusion.

In conclusion, let me urge the importance of two things in the treatment of all non-operative head cases: (1) Extreme and immediate care in the antiseptic treatment of all wounds and abrasions of the soft parts of whatever degree; (2) extreme care in the feeding and general nursing. By attention to these points can septic and pulmonary complications be best avoided, and the patient given his best chance.

ARTICLE XVI.

INDICATIONS FOR OPERATION IN HEAD INJURIES.

By EDWIN W. DWIGHT, M.D.
OF BOSTON.

For many reasons it is impossible to treat satisfactorily and thoroughly a subject of such magnitude within the limits that are of necessity set for the papers read at our annual meetings. It is particularly difficult in this case because we have to deal with a subject with which we are all more or less familiar, and as to the details of which probably no two of us entirely agree. The number of head injuries occurring in every practice is considerable; the number appearing at a large hospital is very great. The literature on the subject is extensive and unsatisfactory, and so much at variance that it will be impossible to cover the ground, and perhaps to arrive at any satistactory conclusions, without spending much more time than is at our command.

I shall content myself with drawing certain conclusions from my own experience, and from a study of some 650 cases in which a fracture of the skull was demonstrated, either by autopsy or operation. It will be impossible to give you all the figures upon which these statements are based, and therefore I shall draw conclusions and state as facts matters which, although not generally accepted, are, I believe, proved to be facts by these figures. In approaching so indefinite a subject as head injuries, and one

upon which opinions differ so widely, it is wise, I think, for us to consider for a moment, (1) what they are, (2) what operations are available for their relief, and (3) what evidences may be discovered on external examination demonstrating a condition which would justify operation.

When we speak of head injuries it is generally understood that we mean injuries affecting the brain, or such injuries as may appear to affect the brain. The brain being protected from injury by a firm, elastic covering, is not affected by slight blows which might cause injuries to other tissues. The skull, however, is exposed to violence in many forms, and injuries to it are very common. Injuries to the brain itself must occur either after a fracture of the skull or as a result of sufficient force applied to it to thoroughly jar and lacerate its contents without injuring the skull itself. On account of the form of the skull, fractures do not cause many symptoms and are not in themselves dangerous to life, the danger coming in all cases from injury to, or pressure upon, the brain.

The brain may be injured by tearing by foreign bodies, by detached or depressed portions of the skull or by hemorrhage into its substance; or it may be sufficiently compressed to cause death or serious symptoms by depressed fragments of bone, foreign bodies or hemorrhage into its substance or onto its surface. Operations in cases of injury to the brain are performed to remove foreign bodies or relieve pressure, and it is only when we have reason to believe that foreign bodies or pressure are present that an operation is justified.

Operations for the relief of intracranial pressure or injury are usually spoken of under the general head of trephining, but this term should be used in the same sense as "laparotomy" or "abdominal section." It should be clearly understood that trephining is simply making an incision which allows us to reach the tissues requiring relief or

treatment. An opening through the skull may be made in several ways, most commonly by means of some form of circular trephine, by burr drills or by the use of mallet and chisel or gouge. In the last few years we have heard a great deal of the advantage of the latter methods, the necessity for some change being based on the idea that the use of the ordinary circular trephine requires great exertion and a long time, and on account of the possibility of injury being done to the brain. In the hands of aurists, or others specially trained in the use of the mallet and gouge, it is probably possible to make an opening through the skull with speed, comfort to the operator and safety to the patient, but in the hands of a general surgical practitioner the use of a trephine is much more rapid, surer and safer.

A trephine opening through the average skull should be made within two minutes from the beginning of the scalp incision. I have repeatedly made it in a minute and a quarter, and have on several occasions timed my assistants, they usually taking about two minutes, never over two and a half. Neither is there any serious danger of injuring the brain. The dura may be opened, but this, I believe, is of no serious importance. The opening, made by a one-inch trephine, is sufficiently large to determine the presence of blood clot, serum or pressure from other causes. It is also sufficient to allow the elevation of depressed fractures and the removal of considerable amounts of clotted blood. Such an opening will begin to decrease in size within a comparatively short time, and within a year the brain is fairly well protected. If, for any reason it is necessary to enlarge this opening, it can be rapidly and satisfactorily done with a properly formed and sufficiently powerful rongeur. The operation itself is not associated with any appreciable amount of shock, neither does it require much anæsthetic, and consuming so little time does not in itself diminish the patient's chance of recovery. For this reason

I consider it a perfectly justifiable exploratory operation in cases where the condition inside the skull is in doubt, and do not hesitate to use it as such with greater freedom than exploratory laparotomy.

The 650 cases which have been referred to, and a study of which has confirmed my opinion in regard to many points in connection with head injuries, are drawn from the following sources: Two hundred from the literature of the past five years; 300 taken from the records of the Boston City Hospital; and 150 autopsies occurring in the practice of Dr. F. W. Draper as medical examiner. These are all consecutive cases in which fractures of the skull were found at autopsy or operation.

The symptoms and external appearances found in cases of injury to the brain may be divided into two classes: (1) Those which show interference with the function of the brain, such as paralysis and condition of consciousness, and (2) those which are the result of injury to the skull, such as external hemorrhage and ecchymoses. As has already been said, injury to the brain not infrequently occurs without fracture of the skull. In such cases operation must depend on the signs presented of interference with the brain itself. In the much more common class, where a fracture of the skull also exists, both varieties of symptoms may be presented for consideration.

FRACTURES OF THE SKULL.

Taking the whole number of fractures of the skull we find that 40% of them were depressed, this number being somewhat larger than probably occurs in fact. Localized, depressed fractures are extremely rare, but 3½% of the whole number were of this character. In every other case there was a fissure extending into the base. Localized fissures of the skull are still more rare, something over 1½% only being of this type. The base is involved in 95% of all

fractures. As it is very generally accepted that depressed areas of bone should be elevated I shall not further consider depressed fractures.

Fissures extending from some portion of the vault into the base may involve one, two or all three fossæ. In 55% one fossa was affected, in 36% two, and in 6% all three. The anterior fossa was involved in 42%, the middle fossa in 82%, and the posterior fossa in 25%. By far the most common fracture is one beginning in some portion of the parietal bone, a fissure extending either along or across the middle meningeal grooves, and ending in one of the foramina. When the fracture is a depressed one its location may usually be recognized, but when, as is more commonly the case, there is only a fissure, we must depend upon less definite evidences, the most important being external hemorrhages from the ear, nose or mouth, or hemorrhage under the scalp or about the eyes. If in the form of a fissure, injury to the bone itself does little harm, and rarely gives rise to any symptoms, those we do find simply depending upon the external appearance of hemorrhage from within.

When a well-defined hemorrhage under the scalp or into the back of the orbit occurs after an accident where violence has not been directly applied to the point of hemorrhage, it may be accepted as positive evidence that a fracture exists near that point. When hemorrhage occurs from one or both ears the probabilities are in favor of a fracture on the side from which the hemorrhage comes. In 67% of these cases in which there was a hemorrhage from one ear a fracture of the middle fossa on that side was found. Hemorrhage from the nose is of much less importance. In only 22% with bleeding from the nose where violence had been applied elsewhere was there found a fracture of the anterior fossa. When such external hemorrhages are absent it must not be assumed that there was no fracture through the adjoining fossa, as in 31% of fractures of the middle fossa no hemor-

rhage from the ear was found, and in 30% of all fractures there was no hemorrhage of any kind. The diagnosis of a fracture in the skull depends upon its discovery by touch or the presence of one of the forms of external hemorrhage, and when so large a number is found in which a fracture exists without either of these manifestations it must frequently be impossible to be sure of their presence if they do exist.

The diagnosis of injury to the brain usually depends upon the condition of consciousness, condition of the pupils and pulse, and the presence or absence of some form of paralysis. In those cases in which there is a decided variation from the normal in one or more of these several conditions, it is usually safe to say that there is some form of interference with the function of the brain. Unfortunately, for the ease of diagnosis, in a very large proportion of cases there is no change from the normal, and when such a change does exist, while it may show the presence of some intracranial disturbance, it does not help us materially in deciding just what has gone on within the skull.

The condition of consciousness is one upon which much stress is laid, and very false ideas seem to be common as to the importance of a condition of coma, either progressive or otherwise, in deciding the question of operation. Profound and absolute unconsciousness is extremely rare in cases of head injury, in but 5% of cases is it present, and then more commonly when the injury is in the ventricles or basal ganglia. The percentage of cases in which the individual is thoroughly conscious is also small,—about 11%. Most common are those which have varying degrees of consciousness and restlessness, and it is among these that the thoroughly operable cases are found, and most mistakes are made. It is very common to have that condition described as alcoholic stupor or alcoholic delirium, and as many of our hospital cases that have received an injury to the head have an alcoholic breath, the diagnosis of alcohol is very frequently made.

The condition of the pupils may be of assistance in deciding the question of operation, but unfortunately the number is very large in which marked intracranial disturbance exists without any characteristic change in the pupils. The Hutchinsonian pupil, that is, dilated on the same side as the hemorrhage and contracted on the opposite side, is frequently spoken of as of importance in the diagnosis of extradural hemorrhage. I do not find that it is more common when the hemorrhage is extradural than when it is subdural or into the substance. When it does occur it is a fairly accurate test as to which side the hemorrhage is on, but it tells us nothing otherwise as to its location, and it is only found in 10% of all cases. I find, in a general way, that when a single pupil is dilated the hemorrhage is on the same side in 70%, on the opposite side in 30%; and when a single pupil is contracted the hemorrhage is on the same side in 30%, on the opposite side in 70%.

The character of the pulse has a certain value, but has not nearly that with which it is ordinarily credited. Normal and rapid pulses are not at all rare in cases of more or less extreme cerebral pressure, and when these conditions are present they tell us very little as to the character of the cerebral injury. The pulse was found to be slow in something over 40% of all cases and to have the bounding character in 27%. When it is slow, or slow and bounding, the chances are largely in favor of an extradural hemorrhage, 90% of all slow pulses in this list of cases being associated with extradural hemorrhage.

When a definite paralysis is present, it must be accepted as one of the most valuable signs that we have as to the portion of the brain that is affected. It shows very little, however, as to the character of the hemorrhage or injury. If we can judge anything by the figures at our command, paralysis is much more common in the cases in which the brain is injured than in purely extradural or subdural

hemorrhages, and it is most rare in the extradural. In the majority of cases of extradural hemorrhage of slow onset paralysis does not occur. Where it is found within a short time after the accident, we may assume that there is either a hemorrhage into the substance or laceration in the region of the centre affected. Paralyses appearing later are more apt to be due to pressure alone.

The presence or absence of the various reflexes is supposed to be of some importance in deciding the question of cerebral injury. From a study of these cases it has been impossible to arrive at any definite conclusions. It has, however, been noticed that the diminution or absence of reflexes was almost, if not quite, constantly associated with profound degrees of unconsciousness, coming on promptly after the injury, and so would appear to have comparatively little value, unless it is to indicate a considerable degree of injury to the more important structures of the brain.

The decision as to operation in doubtful cases is not materially helped by the presence or absence of all or any set of symptoms. The presence of definite, localizing symptoms, especially those such as coma and paralysis, point toward hemorrhage into, or injury to, the brain substance, and are not usually found in those cases which will most probably be relieved by operation. Those cases that are most distinctly operable, that is, extradural or subdural hemorrhages limited to one side, are not usually associated with definite symptoms.

Personal experience, and a study of all the cases that are available, have apparently demonstrated that the symptoms presented in the majority of cases of injury to the brain are ill defined and not such as we might expect from a study of the literature; that they are best defined in those cases that are least apt to be benefited by operation; that in the vast majority of all cases of injury to the brain, and especially in those such as one-sided extradural or subdural

hemorrhages, where an operation may be expected to give relief, the symptoms are indefinite and may be entirely wanting.

In distinctly operable cases, which include about 40% of all deaths associated with fracture of the skull, we usually find at autopsy a fissure beginning in the vault and running into or through the middle fossa, death being due to pressure from extradural or subdural clot. These cases may or may not be associated with varying degrees of cerebral laceration. A careful study of the symptoms in such cases will show that there probably was good reason to suspect some form of intracranial disturbance, but little, if any, definite proof.

Of the fracture itself we may have some definite sign, either of depression or that a crack exists from which a deep hemorrhage appears. If such a hemorrhage exists, either in the ear, nose, under the scalp or in the post-orbital space, it is safe to assume that a fracture will be found in that region. In a very large number of cases we simply find indefinite signs of cerebral disturbance without any localizing symptoms, either of fracture or injury to the brain. In these cases it is my habit to do an exploratory trephining, and a very large proportion of them are found to be those which most definitely require operation.

In the dim light afforded by these facts I lay down the following rules: Given positive signs of intracranial disturbance following an accident which might well cause such a condition, but without definite signs of fracture, I should explore. Given a case in which there is probable evidence of fracture, and a probability of interference with the brain, I should operate.

I have never seen fatal or serious results follow trephining by skilful hands, even when two or three openings in the skull were made, and I have seen many lives saved by exploratory operations in this very large class of doubtful head injuries.

ARTICLE XVII.

INDICATIONS FOR OPERATION ON HEAD INJURIES.

By WILLIAM N. BULLARD, M.D.
OF BOSTON.

Previous to the introduction of antiseptic surgery, and indeed until within the last twenty years, the only conditions which were generally believed to call for operative interference in head injuries were compound depressed or compound comminuted fractures of the cranium.

With the general use of antisepsis and, later, asepsis, the comparative safety of operations was immensely increased, and it became natural to inquire whether operations could not be undertaken with benefit in other forms of head injury.

The general interest taken in the later eighties and early nineties in brain surgery, and the greater frequency of operations on the brain, drew attention to the fact, which was contrary to the preconceived views of the time, that under aseptic conditions the meninges and the brain could be incised, punctured and manipulated with comparative impunity. I shall never forget how, in one of the earliest cerebral operations in which I was professionally interested, I saw one of our best-known surgeons plunge a bistoury into the optic thalamus, and the mental relief when no symptoms whatever followed.

The comparative safety of simple incisions of the brain being proved, there was still much discussion in regard to

the dangers of interference with the meninges, and many physicians and surgeons were convinced of the serious results likely to follow upon an incision or a laceration of the meninges. In consequence of this opinion many operations were only partially performed, the meninges being left intact after the trephining, and the results being thus impaired. The question of the safety of incision of the meninges, and whether such procedure increases the severity of the operation and endangers the recovery of the patient, has now in my opinion been definitely settled. Aseptic incision of the meninges is not in itself dangerous or even very serious. It is imperative in many cases of brain injury if the full benefit of the operation is to be obtained.

What are, in the present state of our knowledge, the indications for operation in head injuries? Opinions differ somewhat in this matter. Some surgeons go so far as to advocate operation in all cases of fracture of the cranium, and in many cases of prolonged unconsciousness following injury to the head. Others are more conservative, and would advise operation only in those cases where it is obviously and imperatively demanded.

In answer to this question I shall consider the more common signs and symptoms of head injury in detail, and discuss the value of each by itself and in combination with others.

FRACTURE OF THE CRANIUM.

I. How far is fracture of the cranium in itself an indication for operation? This depends on the character of the fracture, on its position and on the age of the patient. I will speak only of fractures of the *adult* cranium.

Compound depressed and compound comminuted fractures should be operated upon in practically all cases. (This, of course, presupposes, as we do throughout this paper, the presence of suitable conditions for operating and

asepsis.) All punctured or perforating fractures come under this class. I believe that in all bullet wounds of the skull from the outside, that is, not through the mouth, nose or ears, or through the base of the skull, where there would be excessive difficulty in reaching the opening, the bone around the bullet wound should be trephined, not for the sake of finding the bullet, but simply for the purpose of cleansing the wound, as far as may be, and allowing no secretions to collect about the meninges or between them and the bone. In compound fractures of the cranium where there is no depression and where only a single fissure is found, it is safer to operate (trephine) even though there be no other symptoms.

Simple fractures. — What shall we do in cases of simple fracture without symptoms? This is the class of cases about which opinions differ most strongly. I believe that on the whole it is wiser and better in these cases to be conservative and not to operate, but the patient should be kept under careful observation and operation be undertaken at once if any indication presents itself. Investigations on the results of simple fracture of the cranium show that only in rare cases are they serious, or such as could have been relieved by operation. I wish here to emphasize certain facts. Intracranial complications may arise with any severe head injury. Deep cerebral hemorrhages, single or multiple, may exist. We may have single hemorrhages situated in the basal ganglia or in other inaccessible parts of the brain, or we may have numerous minute or microscopic hemorrhages scattered throughout the brain substance. Neither of these conditions are operable, and their symptoms and results are not to be confused with those of simple fracture. Again, in all cases of severe blow upon the head, and probably in practically all cases of fracture of the skull, there is more or less contusion of the brain. This is apt to be on the opposite side from the direct blow. It is prob-

ably often the cause of certain disquieting symptoms, and its presence must not be forgotten.

The later conditions resultant from the fracture itself in cases of simple fracture of the cranium are not usually marked. If in any given case epilepsy should occur after an injury producing simple fracture of an ordinary character, an exploratory trephining should be resorted to at once after the first attack, but such condition is rare (or absent) in cases of simple fracture without concomitant symptoms of pain, unconsciousness or fever.

LOSS OF CONSCIOUSNESS.

II. The value of loss of consciousness as an indication for operation in head injuries varies somewhat in accordance with the degree to which it exists and, above all, with its duration. We find in these cases all shades, from that in which the patient is but momentarily dazed to deep stupor lasting for days or weeks. The severer grades are often accompanied by extreme restlessness and sometimes by delirium. These conditions will be considered later.

In the first place, the absence of any marked degree of unconsciousness does not contra-indicate operation. Such a condition may exist with very extensive cranial injury. I recall a laboring man who was struck in the head with a pickaxe, in a town about seventy miles away, early one morning. He had no loss of consciousness. He took the train to Boston that day, arriving here in the evening, waited and got his dinner, and then walked to the hospital about half-past eight in the evening. On examination a most extensive comminuted fracture, involving about half of one side of the vertex, was found.

Secondly: The degree of unconsciousness does not in itself determine the necessity of operation. In many hospital cases the cause of the loss of consciousness is not uncomplicated. Alcohol plays an important part, and the

influence of this factor must be eliminated. It is well known that a blow on the head of a person addicted to, or under the influence of alcohol, will induce symptoms due to the alcohol which would not have been apparent without such exciting cause. I need hardly mention the occurrence of delirium tremens after injury as an example of this. This affection is usually too apparent to be overlooked. An injury to the head in an alcoholic person, or one under the influence of alcohol, often produces a result which neither the alcohol nor the injury alone would produce. The effects of the alcohol previously latent, suddenly reveal themselves taking advantage of the lessened power of cerebral resistance, and the loss of consciousness is deepened—an alcoholic stupor is added to what might be called the simple or uncomplicated condition due to fracture. Such a patient shows a greater degree of unconsciousness than would naturally occur in a patient free from alcohol who had suffered a like injury, and the unconsciousness in alcoholic cases seems to last longer than in the average non-alcoholic patient. Whether this is a fact can only be determined by considering large numbers of cases, because, as before stated, the degree of unconsciousness is *not* proportionate to the severity of the cranial injury.

Alcohol taken in excess previous to the injury (and this excess need not be such as to be apparent) not only tends to increase the degree of unconsciousness, but also produce delirium. The low, muttering delirium so often seen in older men after a head injury, is usually due to this and may last for days or weeks. To this also is due the temporary delirium and peculiar actions in certain cases which occur at various times in the day, the patient being quite natural in the intervals.

Though the degree of unconsciousness is not proportionate to the severity of the cranial injury, the duration of the unconsciousness is so to a certain degree. I am convinced

that in all cases of uncomplicated head injury in adults, where alcohol and other brain lesions, such as paralytic dementia, can be excluded, in which the patient remains unconscious for over twenty-four hours, operation should on this ground alone be seriously considered. Such a condition points to serious intracranial injury—very possibly a large epidural clot.

Cases occasionally occur in which without intervention the patients remain unconscious for two weeks or more. Experience seems to show that the results would be better were these patients trephined early, and that even a late trephining is advisable. It is scarcely necessary to say that renal and all other complications must be excluded.

Delirium in these cases, if not accompanied by restlessness and fever, and if not due to alcoholism, is exceedingly suggestive of chronic renal disease. When accompanied by restlessness and a high temperature, exploratory operation may be considered. Under these conditions, however, we are rarely without more positive indications to guide us. If delirium, fever and restlessness without other symptoms are found after head injury, the indications for operation are not positive.

Temperature.—When there is a marked rise of temperature shortly after a head injury, and especially when the temperature continues to increase, the prognosis is more or less serious. If not due to complications of other organs it suggests serious injury to the brain and a grave prognosis. It is not a contra-indication except so far as it indicates that operation is not likely to remedy the trouble completely, and that a condition not to be relieved by operation, exists. Subnormal temperature after head injury is frequently due to shock. When not due to shock a subnormal temperature is not in itself a contra-indication to operation. Subnormal temperature without unconsciousness or other symptoms is of no definite importance.

Shock being eliminated, a long-continued sub-normal temperature in an unconscious patient suggests œdema of the brain without severe injury or laceration of the brain tissue. If this œdema is not due to alcoholism, paralytic dementia or renal disease, operation should be considered twenty-four hours after the accident.

Pain.—Acute, severe pain is always a symptom deserving serious consideration in head injuries. In connection with other symptoms it may serve as an indication for operation. In any case where there is severe pain in the head lasting over twenty-four hours, operation is to be considered and a most careful examination made for other symptoms. I can recall at the present moment a case in which a patient was brought to consultation solely on account of persistent severe headache following an accident in which he was thrown out of his wagon. There was no unconsciousness, no fever, no delirium. The patient was able to use his limbs, and except for his headache might have been considered to be in normal condition. The only definite sign found was a dilation of one pupil. It was in this case determined to operate, and an extensive fracture of the cranium was found.

On the other hand, we must be on our guard against functional pain dependent on the general temperament of the patient, rather than on local conditions. A person of neurotic tendency may have severe pain in the head following head injury due in large part to his nervous condition, or aggravated in consequence thereof. In these cases operation is not indicated by the existence of pain. The diagnosis between the forms of pain must be made from the general condition of the patient.

We now come to the more striking symptoms of brain *irritation* and *inhibition*, which are often also of value as suggesting or determining the position of the injury.

CONVULSIONS.

Convulsions may be tonic or clonic, and in either case general (diffuse) or localized. (In the adult, tonic convulsions unaccompanied by clonic are not common in the early periods after head injury.) General tonic convulsions are rare. They would suggest either some complicating affection or possibly an injury in the region of the posterior fossa or of the medulla. Localized tonic convulsions when they occur are apt to alternate with clonic convulsions of the same region, and suggest an irritation of the corresponding intracranial regions.

General Clonic Convulsions. — These are almost always due to some special condition of the patient or some complication. When they exist epilepsy is to be suspected. Epilepsy itself may be the determining cause of the injury, or the shock of the injury may bring on an epileptic seizure. The possibility of its existence can never be neglected in a case where the previous history is not fully known, and where convulsions of this character exist. In these cases the convulsions are accompanied by unconsciousness.

When general convulsions occur while the patient is conscious or semi-conscious, especially if of the female sex, hysteria or functional trouble must be considered, and this must be eliminated. General clonic convulsions may possibly be caused by alcoholic conditions alone, but I consider this doubtful. They may be due to uræmia, which must always be considered before operation is undertaken.

Clonic localized convulsions are important both as determining the advisability of operation in many cases and the place of the lesion. They are not, however, an absolutely certain sign, either of the position of the most serious intracranial injury or of the necessity of immediate operation, but are only suggestive. The most common form of localized convulsions is the hemiplegic, when they involve the muscles of the limbs, and often those of the face on one

side — the side opposite the brain lesion. When definitely localized in one limb, or in one portion of a limb, their significance as showing the exact localization in the brain is greater than when larger areas are involved. Thus, convulsions localized in a thumb, or in certain fingers, or the forearm, would have a great localizing value; but, unfortunately, these closely localized convulsions in the limbs rarely occur. I do not, on the other hand, attach much significance to the affection of certain muscles in the face. The facial muscles seem to be readily affected in many cases. The ocular muscles often show the first signs of the commencing convulsions, whether hemiplegic or general.

It is rare that a special small series of muscles is alone affected throughout a convulsive attack. Many attacks begin with convulsive movements of a few muscles, then of more, and finally nearly all the muscles are affected. Formerly, we attached considerable importance to the determination of the group or groups of muscles first affected in these cases, and this should always be noted, but experience has shown that the importance of this symptom has probably been over-estimated. We find that in several successive attacks different groups may be involved first. We find that the gross lesions involve or affect many centres, and apparently the most sensitive, and not necessarily those most affected are first involved.

Convulsions are of great diagnostic importance in determining the side of the lesion, that is, which half of the brain or its membranes is affected. For closer localization they are apt to be deceptive. This is the rule. There are, however, certain exceptions or limitations thereto. (1) They may also be produced in œdema of the brain in alcoholic patients in cases where at autopsy no gross lesion whatever (except œdema) is found; (2) the side of the brain so affected as to cause convulsions may not be the side of the fracture or of the greatest apparent gross injury. They may be produced by the *contrecoup*.

Hence, when the other symptoms are sufficiently decisive, the evidence of the locality of the injury given by convulsions is to be neglected. For example, if the convulsions, as is sometimes the case, are on the same side as a compound depressed fracture, operate over the fracture. It may not be advisable to operate on the other side at all.

PARALYSIS.

Under this heading we include not only all forms of total paralysis, but also the cases of partial paralysis or marked weakness of muscles.

Inequality of the pupils is always important. This may, of course, be natural to the individual; it may be due to local eye lesions, cataract, etc.; it may be due to syphilis or to other cause existing before the injury. These causes must receive due consideration. If, however, we decide that the evidence is in favor of the condition being due to the injury, we have a symptom of great value in the determination of the situation of the lesion. Unilateral dilatation shows injury on the side of the dilated pupil, and often occurs with fracture of the base on that side. Taken in connection with unconsciousness, this sign, in my opinion, is sufficient to justify operation.

Unilateral myosis, or contraction of the pupil, is much rarer. It would suggest — if not due to some cause unconnected with the injury, as would be most probable — an irritation of the brain on the same side.

Paralysis of the external ocular muscles naturally points to fracture of the base on the same side as the muscles affected. Paralysis of the external rectus is due to injury of the sixth nerve. Paralysis of the internal rectus to that of the third. Paralysis of the fourth nerve is practically not distinguishable in the cases seen immediately after the injury. Paralysis of both sixth nerves suggests fracture of the base on both sides, and very serious injury. I will

merely mention that great care must be taken not to consider a previous strabismus as due to the injury.

Paralysis of the face alone may be due to injury of the seventh nerve when all three branches of the nerve are involved, or to a central lesion when only the lower branches are affected, and the wrinkling of the forehead continues essentially normal. When the lesion involves the nerve it is on the same side as the paralysis. When it involves the brain it is on the opposite side. Nuclear paralyses are so rare that they may be neglected. It is quite possible that the seventh nerve may be injured through the ear.

The most striking form of paralysis, undoubtedly, is hemiplegia. This term is here used to denote paralysis of both limbs on the same side of the body. This points to a lesion on the opposite side of the brain. It may be associated with paralysis of the face on the same side as the hemiplegia, which suggests a more extensive lesion than the affection of the limbs. It may be associated with paralysis of the face on the opposite side, but much more rarely. This is only mentioned to state that when it occurs it may be due to a lesion on the opposite side from the hemiplegia, involving the seventh nerve, and that this is more probable than a hemorrhage in the pons. A hemiplegia may be associated with paralysis of many other muscles, especially the external ocular muscles, but we cannot enter into consideration of these combinations here, as it would lengthen this paper unduly.

The significance of hemiplegia as an indication for operation is great. It might at once be said that when it occurs in adults as direct result of the cerebral injury operation should always be performed unless some special contra-indication exists. This is true whenever the hemiplegia results directly from the injury itself. But, unfortunately, we have a considerable number of cases in which the hemiplegia is not the immediate direct result of the injury, but

is caused by the cerebral œdema which follows, and this condition is specially common in the alcoholic. It is not always easy to distinguish in the unconscious patient the hemiplegia due to an alcoholic œdema from that caused by a direct lesion. In my opinion one point is of special value in this connection. The paralysis in œdema is rarely or never total; it is almost always a weakness or decided weakness, while some power of voluntary motion still remains. Operate, therefore, in all cases of hemiplegia in adults not due to alcoholic or uræmic œdema. Operate, as a rule, in doubtful cases, because even in œdema the opening of the meninges undoubtedly tends to benefit the patient, but in such cases be prepared not to find a clot or fracture, or other visible lesion.

Aphasia, when organic, suggests brain injury or pressure. It usually occurs in connection with unconsciousness or some form of paralysis, more especially hemiplegia.

I shall not consider now the question of operation in the head injuries of children, except to say that with them we should be much more conservative. Children recover without serious results from lesions which would produce much more severe after-effects in adults. The brain apparently adapts itself to a certain amount of localized pressure. Hemiplegias do not have the same serious significance.

SUMMARY.

(1) Operate in all cases of compound depressed and compound comminuted fracture of the cranium. It is usually advisable to operate on any compound fracture.

(2) Simple fracture of the cranium without symptoms does not as a rule demand operation.

(3) Absence of unconsciousness does not contra-indicate operation. The degree of unconsciousness is not in all cases proportionate to the severity of the injury.

(4) The duration of unconsciousness is important, and

when it lasts more than twenty-four hours—no other cause than the injury being present—operation should be considered.

(5) Marked rise of temperature after uncomplicated head injury suggests serious injury to the brain. It is not necessarily an indication for operation. A subnormal temperature without other symptoms has no special significance. When accompanied by unconsciousness and lasting twenty-four hours or more it suggests œdema of the brain or intracranial hemorrhage.

(6) Severe pain in the head continuing for some time after a head injury, if organic, indicates operation. Pain in the head following injury may, however, be functional and due to general nervous conditions.

(7) Convulsions, when clonic and diffuse, suggest epilepsy or other complication. When localized they are of value as indicating the side of the brain on which the lesion producing them is situated. Taken in connection with other symptoms their presence usually favors operation.

(8) The presence of paralysis of the limbs in adults, if marked, usually indicates immediate operation. Partial hemiplegias and paralyses of the limbs may occur in edema of the brain following injuries.

(9) The above statements refer to adults only. In children paralyses are more apt to pass away and the indication for operation is not so decided.

DISCUSSION.

INDICATIONS FOR OPERATION IN HEAD INJURIES WITHOUT FRACTURE.

Dr. G. L. WALTON, of Boston: I fear I should hardly do justice either to myself or to a subject of so great scope and practical importance if I attempted to discuss without

familiarizing myself with their contents papers prepared by such accurate observers and painstaking collaborators. I shall venture to present rather a résumé of the principal rules for guidance which seem justified by my own experience in that branch which falls particularly within the province of the neurologist, namely, head injury without sign of fracture.

In these cases the question *whether* we shall advise operation generally resolves itself into determining if hemorrhage is present, and if so, is it accessible, and is it increasing. The question *where* shall the trephine be applied is determined by the seat of the hemorrhage. As in most affections of the central nervous system the seat of the lesion is determined largely by the physical examination, but for its character and course we rely principally on the history.

And it is often the character and course of the lesion which prove baffling in these cases through lack of reliable history or even of any history at all.

A patient is brought to the accident room unconscious or semi-unconscious, restless, perhaps hemiplegic. The seat of the lesion may be often rapidly narrowed by physical examination thus—Impairment of consciousness—brain; hemiplegia—opposite side of brain, etc; but when we ask "Did this come on suddenly or gradually; were there prodromata signifying that apoplexy caused the accident; was there an interval of consciousness; was the fall or blow sufficient to produce hemorrhage; had he previous symptoms of tumor, or of renal disease; was he intoxicated or poisoned; was he alcoholic?", we are answered—"He was found in the street in this condition, and that is all we know about it."

Fortunately the probable lesions in accidents without fracture are limited to hemorrhage, bruising, œdema and alcoholism, or perhaps to more than one of those pathological conditions combined.

What are we to do in this event? In most cases wait till we can establish at least a certain amount of history by our own observation, aided by that of a nurse instructed to look for twitching, deepening of unconsciousness or other symptom of increasing hemorrhage. (For while I agree with Dr. Dwight that the mere presence or absence of unconsciousness is not a question of the first magnitude, the

increase or *decrease* of such unconsciousness is a matter of the greatest importance.) The occasional case in which this policy will lose us an opportunity for successful immediate operation will be overshadowed by those in which the outcome will prove that such operation would have been either futile, or needless and perhaps harmful.

In such a case the symptoms may disappear in a week or may merge into typical symptoms of delirium tremens—diagnosis,—no hemorrhage, the important question as regards operation. Whether the lesion was bruising, œdema, alcoholism, or all three, is not material for this discussion.

On the other hand, the unconsciousness deepens—the paralysis spreads and perhaps rigidity or clonic spasms appear and become general. Diagnosis—hemorrhage still increasing; operate, in the hope of finding it superficial and accessible, whether over or under the dura. Choose for seat of operation the region of the centre of the parts originally paralyzed, but do not make the field of operation too narrow. Remember, too, that original monoplegia favors the cortex, while immediate paralysis of face, arm and leg points either to the internal capsule, or to very extensive cortical or subcortical hemorrhage. Watch the pupil on the well side meantime—its dilatation means middle meningeal artery and involvement of the third nerve—look out also for the Babinski sign, which points inevitably to pyramidal involvement. Do not rely too much on the pulse or temperature; operable hemorrhage may be present without, for example, the slow pulse upon which so much stress is laid in classical descriptions. Again, suppose the patient is not hemiplegic at first, that he recovers consciousness and gets about, but after a few days or a week, has twitching of the face or hand, gradually increasing and spreading to other parts with deepening unconsciousness—this is the classical history of meningeal hemorrhage; operate, the sooner the better, and of course, if this is the history when the patient is first seen, operate at once. If just these symptoms appear operate even if three months have elapsed, or even if no accident has occurred. Do not hesitate to operate because the patient is moribund, with stertorous respiration and convulsed in all extremities, provided the onset of the symptoms corresponded with meningeal hemorrhage. I recall cases of hemorrhage restored to

perfect health by operations performed under these circumstances by Drs. Homans and Mixter, and of abscess by Dr. Jack. But if general convulsions and stertorous respiration appear at the outset, operation will be unavailing. This decision was verified, for example, in a case of death after a blow on the jaw in boxing, in which the post mortem showed laceration of the lateral sinus with flooding of the middle fossa and extension of hemorrhage in every direction.

In another case the mind clears but paralysis remains unchanged. Diagnosis—self-limiting hemorrhage into the substance of the brain, perhaps in the cortex (probably so if the paralysis is limited) but not justifying operation, especially as it may absorb. (Such a case I have recently seen with Drs. Warren and Williams in which paralysis of one foot, with Babinski, appeared early, persisted several weeks and finally disappeared.)

Remember that paralysis may be due to pressure of the uninjured hemisphere against the skull, and trephine the other side as well as the side indicated in case the symptoms clearly point to increasing hemorrhage.

Remember that children are prone to become hemiplegic or hemiplegic immediately after blows on the head, probably from serous effusion into the arachnoid, a process analogous to the formation of a blister. Such paralysis disappears in three or four days, as in a case of Dr. Boland's (monoplegia), and one of Dr. Percy, seen by Dr. Scudder and myself (hemiplegia.) That a similar pathological condition may obtain in adult life was seen in a case reported by Dr. Brooks and myself in which paralysis of the arm followed a fall on the head. Operation over the centre indicated gave rise only to a spurt of serous fluid as soon as the tense dura was incised.

Remember also that hysterical lethargy may follow a blow on the head. This I have seen in one case with Drs. Richardson and Mumford, and another with Dr. Scudder. This uncomplicated traumatic hysteria gradually wears away. Such experiences should lead us to wait and watch the prognosis of doubtful cases before advising operation, particularly in children and young women.

If so-called traumatic delirium appears we should give the benefit of the doubt to alcoholism, even if not acknow-

ledged or not recognized by the friends, and should not operate unless the indications are most decided.

Finally, it is in case of multiple paralyses or spasms and other baffling symptoms that our personal equation comes in play. We may have on the one hand to do with extensive though unrecognized fracture, or with such varied and extensive hemorrhage that operation would be futile, or on the other hand, this may prove one of those cases in which complete recovery shows the lesion to have been nothing more serious than bruising, œdema, alcoholism, or all three. In case of such apparently serious head injury the operator will have no cause for blame if he decides to make exploratory operation, though unsuccessfully, nor if he fails to operate will he often be confronted by a post mortem which causes regret that he held his hand. My own personal predeliction in cases without fracture is to advise operation only when the indications are fairly clear, but in case of fracture, or even of probable fracture, to give operation the benefit of any reasonable doubt.

Dr. BULLARD: I should like to call attention to the fact that in a certain class of cases after injury an aphasia sometimes develops which is hysterical in character and great care should be taken not to mistake that form of aphasia for organic aphasia due to injury. A short time ago a man was brought into the hospital apparently unconscious after a fall of fifteen feet from a bridge into cold water. On recovering consciousness he was found to be aphasic. There was no other paralysis whatever. That case turned out to be entirely hysterical. The man remained aphasic three days and with encouragement recovered. He left the hospital well.

ARTICLE XVIII.

GUNSHOT WOUNDS OF THE HEAD AND CRANIUM.

By LOUIS A. LaGARDE,
MAJOR AND SURGEON U. S. ARMY.

THE introduction of steel-armored projectiles of reduced calibre for the military rifle of the civilized nations has modified the subject of gunshot wounds in nearly all the tissues, the head included. The experimenters with this new high-velocity bullet that does not deform on impact noticed in the beginning that the amount of destruction conferred by it at close range was very great, and that this destruction was generally proportional to the velocity and the resistance encountered on impact. When these conditions of velocity and resistance obtained, the destruction took the character of what is often designated as explosive effects. These so-called explosive effects were especially noted in fluid or semifluid tissues enclosed in rigid walls, of which the brain and skull are the most typical examples. Such a lesion is usually characterized by a wound of entrance corresponding in size to the calibre of the projectile inflicting it, and a wound of exit varying in diameter from one to several inches. The channel track between the exit and entrance wounds is much lacerated and marked by the presence of bony sand and spiculæ of bone varying in size from one-fourth to one-half inch. The latter are usually found to have been driven into the tissues, not only in the line of flight of the projectile, but at right angles to it, as

if an internal force had exerted itself in all directions. Many theories have been advanced to explain explosive effects, but it is now pretty generally attributed to the transmission of the energy of the projectile to the particles of bone and other tissues that become detached and then act as secondary projectiles.

In the middle and remote ranges where the remaining energy has become very much less, the destructive effects in wounds of the head by the new bullet are very much less than those inflicted by the old large leaden bullet that was apt to deform on impact.

Gunshot wounds of the head are usually divided into flesh wounds and fractures. The latter are necessarily very fatal. In the Civil War the fatality among all head wounds that reached the hospitals was 28.93%. The fatality of the same class under similar conditions in the Spanish-American War was about the same, 26.09%. The figures concerning the relative number of the two are shown in the following table.*

	Seat and character of injury.	No. of Cases.	Percentage.	Per cent. of all wounds.		Recoveries.	Deaths.	Undetermined results.	Per cent. of fatality.
	Head:								
Civil War.....	Flesh wounds.	7,739	64	3.14	4.9	6,573	2,676	2,480	28.93
	Fractures.	4,350	36	1.76					
Spanish-American War.....	Flesh wounds.	40	56	2.74	4.86	42	18	2	26.09
	Fractures.	31	44	2.12					

The figures illustrate the frequency of glancing shots with the slow-velocity leaden bullet of the Civil War as compared to their infrequency with the high-velocity steel-

* Extract from Borden's Military Surgery, Prize Essay, Proceedings Association Military Surgery, 1900.

jacketed bullet in use at present. The latter, as a rule, travels in a straight line from the point of impact, being seldom deflected from its course by resistant bone. The relative increase in the number of fractures from the use of the modern rifle augments the fatality in head wounds, so that the humane features attributed to this weapon, already referred to, do not apply to wounds of this region. These results were foretold by the experimenters before the opportunity to observe the effects of the new arm in the actual conditions in war had presented themselves. Stevenson,* writing in 1897 upon the proportion of flesh to fractured wounds of the skull, states as follows: "In future wars the proportion will be very different. Omitting indirect and grazing hits, almost every bullet which hits the skull, up to a range of 2,200 yards, will perforate it, and at about 2,900 yards they will penetrate and remain lodged. The destruction produced in the skull and brain by modern bullets is so extreme, even at long ranges, that comparatively few patients will survive long enough to reach the field hospital for treatment, and for the large majority of those who do, treatment will be unavailing."

Concerning the results in the Anglo-Boer War, Makin† states: "Injuries to the head formed one of the most fruitful sources of death both upon the battlefield and in field hospitals." He mentions some "sensational immediate recoveries" among head wounds, and they doubtless were of the character of those noted by us in the Santiago campaign, namely, oblique perforations of the skull cap involving the brain tissue to a limited depth. The gunshot fractures about the base and those running deeply through the brain from base to vertex, or *vice versa*, were uniformly fatal. Of sixty-eight cases of gunshot fracture which were reported in the Surgeon-General's Report for 1900, and

* Wounds in War, W. F. Stevenson, Colonel-Surgeon, British Army.
† Experiences in South Africa, 1901.

which occurred during the two preceding years, 54.4% ended fatally as against 59.2% out of 2,514 cases noted in the Civil War. Unlike wounds of other regions, those of the head, unless properly treated at once, undergo suppuration, a fact which is not surprising when the amount of dirt overlying the scalp is considered. The surgeons in the Cuban and South African campaigns are unanimous in recommending early and free exploration of all cases of fracture of the skull by gunshot. The aim should be to relieve pressure and to rid the wound of all dirt and loose pieces of bone as soon as possible.

DISCUSSION.

Dr. F. A. Harris, of Boston: Naturally a great many factors enter into the production of the results of gunshot wounds of the head:—the calibre of the weapon; the charge of powder; the age of the powder; the distance from which the shot is fired and also the direction. Moreover a decided influence must be exerted by the part of the skull which receives the first impact of the ball.

I have seen a case where a man fired a bullet of twenty-two calibre into his head at such short range that the hair was scorched. The base of the ball was flush with the external surface of the cranium, but as the ball had entered the petrous portion it had not penetrated the cavity of the cranium and did no more than annoy the would-be suicide, who many hours after firing the shot fulfilled his original intention by drowning himself.

On the other hand I have seen a case where a man fired a twenty-eight calibre ball into his head, holding the weapon so far away that there was *no brand* and yet the top of the skull was utterly shattered, there being as many as thirty fragments, and the ball had buried itself in the wall of the room. This case, to be sure, was that of a Jew, a race whose skulls, according to my personal experience, (whatever may be the general fact) are of most unusual thinness.

There is no sure way of determining the direction of a ball after it has entered the skull. As is well known even so thin a membrane as the pericardium may deflect a ball, and how much more likely is such deflection to be caused by the tables of the skull. To localize a bullet in the head is a task of extreme difficulty, inasmuch as the ragged track made through the cerebral tissue serves to obscure signs which might avail in an injury to the brain of a different sort.

I have found that as a rule the ball is more or less mushroomed in its passage through the bone and from this flattenned and ragged condition far greater injury is done to the cerebral tissue than would be done by the passage of a smooth ball.

I have found the track of the ball through the brain marked not only by disorganized brain substance but also by spiculæ of bone of greater or less size, as well as by fragments of lead scraped from the ball by the sharp edges of the tables of the skull. Even if the bullet could be successfully located and removed, it would be almost practically impossible to cleanse the wound through its entire extent, and remove all these foreign particles which together with the countless punctate hemorrhages would be foci for the starting of an undoubtedly fatal inflammation, allowing that the shock of the original wound was not sufficient to cause death.

I have never observed extensive hemorrhage to result from gunshot wounds of the head doubtless because in those cases where life has been prolonged for any considerable time no important vessel has been wounded, the death resulting later from acute inflammation.

A few remarkable cases like that of Dr. Harlow and that of Dr. Morton demonstrate the miraculous toleration of severe injury to the brain substance, but my own observation of what occurs in consequence of the penetration of the cranium by a bullet makes me in the highest degree pessimistic as to the results of a possible operative procedure with a view to saving life.

ARTICLE XIX.

TECHNIQUE AND AFTER-CARE OF OPERATIONS UPON THE CRANIUM.

By CHARLES L. SCUDDER, M.D.,
AND
HARRY H. HARTWELL, M.D.
OF BOSTON.

Horseley, Wagner, Phelps, vonBergmann, Keen, von-Frey, Lauenstein, Gigli, Codivilla, Braatz, Treves, von-Esmarch, Senn, Macewen and König—these are the men who have recently contributed valuably to the literature of cranial surgery. The work of these men, as well as that of others, has been reviewed in the preparation of this brief communication.

We have been asked to present for your consideration the present generally accepted technique of operations upon the cranium following injury, and to state the method of caring for these operated cases.

OPERATION HAS BEEN DECIDED UPON.

Preparation of the patient.—The evening before the operation a laxative is given; the morning of the operation an evacuating enema is administered. One quarter of an hour previous to the operation ¼ gr. morphia is given subcutaneously. (We are of course supposing that the patient is an adult.) The whole head is shaved, including the eyebrows. It is washed with soap and warm water and scrubbed

with a nail brush. The soap is rinsed off with clean water and then with alcohol. The head is then washed with permanganate of potash (a saturated solution) with a saturated solution of oxalic acid, with a solution (1 to 2000) of corrosive sublimate. Care is to be exercised that none of these strong solutions enters the eye or the external auditory meatus. The corrosive sublimate solution is washed off the scalp with boiled sterile water. The face, neck and ears are protected by a sterile towel fastened snugly about the head. The patient is in the recumbent posture. The head is placed upon a firm surface. It is often convenient to support the head upon sand bags. The parts about the patient are covered with sterile sheets and towels. All instruments, sponges of gauze, sutures and ligatures are properly sterilized. The anæsthetizer prepares himself for the operation as the surgeon does. The proximity of the anæsthetizer to the field of operation demands that every precaution should be taken against his infecting the wound. He should wear cap, face mask and gloves, and use clean cones for ether or chloroform. If the patient be unconscious no anæsthetic will be required. Chloroform or ether will be the anæsthetic employed. There is less excitement and cerebral congestion associated with chloroform anæsthesia than with that of ether.

When a wound of the scalp exists, which possibly communicates with the brain, the same preparation of the head is made excepting that the scalp wound is carefully cleansed by means of small swabs of gauze wet with boiled sterile water, gently but firmly and searchingly introduced into all accessible parts of the wound. The wound is then thoroughly douched with boiled sterile water. It is unwise to use in the wound under consideration strong antiseptics. The bone is thoroughly exposed by enlarging the wound, as may be required.

When there is no wound of the scalp a semi-lunar flap

is raised, the convexity of the flap being toward the median line. The knife cuts directly to bone. The periosteum is removed from the bone by means of a very broad elevator at the same time that the skin flap is retracted. The flap should be ample. A small flap is difficult to work under. Bleeding from the flap is instantly checked by artery pressure forceps. The whole thickness of the scalp is grasped by the pressure forceps. It is a waste of time to attempt to compress each vessel singly. The flap is well retracted by vulsellum forceps (double hooks) or in the absence of these by a suture passed through the free edge of the flap and tied long.

There is a fracture of the cranium.—It is a *fissure;* there is no depression about the fissure. The fissure is followed a distance to determine its direction, its involvement of important cavities—the orbit, the nose, the frontal sinuses and the ear. Continuous oozing of blood from a fissure may mean a serious intracranial lesion. A fissure due to trauma must not be confused with one of the sutures of the cranium. Ordinarily blood can be wiped away from a suture, but it cannot be wiped away from a fissure. The situation of the supposed fissure is helpful in determining its identity. If it is at all likely that dirt has entered a fissure it can best be thoroughly removed by changing the fissure into a gutter by using a small bone gouge held at a great angle with the surface of the bone.

The fracture of the cranium is depressed.—In such fractures the inner table is usually more extensively broken than the outer. The problem presented therefore is not merely how to elevate the depressed fragment or fragments, but how can the depression be relieved without further damage to the underlying sinuses, dura and brain. Necrosis forceps may readily remove loose fragments. The bone may possibly be raised by the introduction of an elevator beneath it.

In a comparatively few cases it will be found necessary to use the trephine. A trephine one inch in diameter is the best for this purpose. The trephine is applied to the sound bone near to the fracture. Two-thirds of the circle of the trephine will be upon the sound skull and the remaining one-third will be over the fractured area. Moving any of the fragments of bone while trephining is to be carefully avoided for fear of injury to the parts beneath. Upon removal of the bit of bone access is had to the parts to be elevated; this is accomplished by the elevator introduced under the fragment to pry it into place. Hemorrhage from lacerated sinuses or other vessels, which perhaps shows itself first when the depressed fragment is ever so cautiously removed, is controlled ordinarily by gauze packing. It may be wise, in order to avoid damage to deeper parts when fragments of bone are much depressed and driven underneath the intact skull, to tréphine over the deeply depressed end of the fragment and thus be enabled to remove such fragment more intelligently. The area from which hemorrhage occurs will then be opened to inspection. It is best to leave only those fragments of bone in the wound that are well attached to periosteum or dura. Completely detached fragments should be removed.

Suppose another case:

There is no evident fracture, and no wound of the scalp.—Intracranial hemorrhage is suspected. The previously mentioned systematic preparation is carried out so far as is possible. The scalp flap is reflected. The 1¼ inch or 2 inch trephine is placed 1½ inches behind the external angular process of the frontal bone and 1¼ inches above the zygona in order to find the anterior branch of the middle meningeal artery from which hemorrhage is suspected. As soon as a groove has been cut throughout the whole circumference of the trephine, the trephine pin (if one has been used) is removed—it has served its duty of steadying the

trephine. The bone dust is dry until the diploë is reached; it is then soft and bloody. It is a good plan while trephining any part of the skull to imagine that it is the thinnest skull in existence and that it is of variable thickness at different parts of the groove. This will tend to make one proceed cautiously. At frequent intervals the depth of the groove should be measured by inserting into it the eye end of a needle held by a pair of pressure forceps. A slight feeling of irregularity will be detected as the needle is carried around the groove; if the inner table has been perforated, the needle will catch slightly instead of moving smoothly. All bone dust is immediately wiped away. As one side of the groove reaches the inner table great caution is needed that in completing the section the dura is not injured. The trephine is tipped slightly towards the uncut portion; greater pressure is put upon this side as the cut is made. Then the button of bone is removed entire. If the button is not easily removed, no attempt to remove it should be made by tipping the trephine and prying it out. A narrow elevator will often remove it in whole or in part, and then the rongeur forceps will complete its removal and smooth the edge of the opening. If the opening made is insufficient it is enlarged by the rongeur forceps. If the hemorrhage is from the meningeal artery the clot is evacuated, the vessel secured by fine silk ligature or a suture of fine silk is passed with a small curved needle through the dura and around the vessel and tied. The bone removed is not to be replaced.

Drainage of the wound should be based upon the same principles as drainage elsewhere. Bleeding from the bone will often cease spontaneously after a few moments; if not the bone may be crushed about the bleeding vessel or a plug of sterile wood or Horsley's wax plug may be used. If there is much oozing of blood or serum or cerebrospinal fluid, and if infection is suspected, drainage is to be em-

ployed. Under other conditions it is not to be used. The wound is best drained by a small, soft rubber tube. The wound in the scalp is closed in whole or in part by sutures of silkworm gut.

If the dura is to be opened, it is incised by the knife and divided by a small pair of blunt-pointed scissors, avoiding so far as possible all vessels. The dura should be divided a quarter of an inch from the bony opening to permit of subsequent suturing, if that is deemed necessary.

The after-care of the operation of trephining. — In uncomplicated cases the patient is kept in a darkened room and as quiet as possible for three or four days. A cold water bag or coil is kept constantly on the head. Calomel in divided doses ($\frac{1}{10}$ gr. each hour for ten doses) is given by mouth. The diet is for the first few days a liquid one. After a week or a week and a half the patient is allowed to be up.

In cases complicated by probable intracranial lesions the patient must be kept quiet a longer time. The wound is cared for according to ordinary surgical principles. If a drain has been used, it must be proved to be efficient. If provisional sutures have been placed, these are tied upon removal of the drain.

Hernia cerebri is the term applied to a protrusion of the brain through the opening left after trephining. This occurs when there is abnormal intracranial pressure. The whole mass of the hernia is not ordinarily brain tissue, but is largely made up of granulation tissue — fungous granulations. It is a most distressing condition which may complicate especially septic cases. Cleanliness, compresses of alcohol and support to the part are indicated.

Concerning the closure of bony defects of the skull, how harmful such defects are, whether pedunculated flaps of periosteum shall be used to close them, or gold foil or rubber tissue or egg membrane or celluloid disks, these questions are still open for discussion.

The value of the Gigli saw is undoubted, and its use in forming osteoplastic flaps for exploratory purposes is of extreme interest, but this does not concern us in this discussion. The electric motors used to drive a saw or trephine are not likely to come into general use.

From Dr. D. A. Green
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Medical Communications

OF

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ARTICLE XX.

THE ANNUAL DISCOURSE.

SURGICAL TUBERCULOSIS.

By HERBERT L. BURRELL, M.D.
OF BOSTON.

DELIVERED JUNE 10, 1903.

NOTE.—At an Adjourned Meeting of The Massachusetts Medical Society, held Oct. 3, 1860, it was

Resolved, "That The Massachusetts Medical Society hereby declares that it does not consider itself as having endorsed or censured the opinions in former published Annual Discourses, nor will it hold itself responsible for any opinions or sentiments advanced in any future similar discourses."

Resolved, "That the Committee on Publications be directed to print a statement to that effect at the commencement of each Annual Discourse which may hereafter be published."

SURGICAL TUBERCULOSIS.

Mr. President and Fellows
of The Massachusetts Medical Society:—

The treatment of tuberculosis is a subject that concerns everyone. All citizens, particularly members of the medical profession, should recognize that tuberculosis is the greatest scourge of mankind and is an enemy that all should strive to conquer. We are apt, in these days of individualism, of unbounded opportunity for personal development, of expert knowledge on special subjects, to consider that the great questions of citizenship, such as concern public health, should be left to the care of the State.

Physicians have recognized, in a somewhat limited manner, that tuberculous patients should be under conditions in which they may avoid a re-infection from the tubercle bacillus, and their processes of repair and powers of resistance may be increased to the highest degree.

Surgeons have not been as keen to appreciate these principles in the treatment of tuberculosis as physicians. Their attention has been concentrated on the treatment of the local lesion. Many surgeons consider that a tuberculous area must be removed; some surgeons regard the area as little less than malignant and feel that a tuberculous lesion must be attacked as if it were a cancer. Relatively few surgeons consider that to increase the patient's power of resistance to

I am indebted to Dr. Wm. H. Davis for valuable assistance in collecting the literature of this subject.

fresh invasions of the tubercle bacillus is of as great importance as to operate and remove a local area of tuberculosis.

A few orthopedic surgeons and genito-urinary surgeons recognize the importance of out-door treatment. General surgeons, however, are too apt to consider that an operation on a local tuberculous area ends their duty. They fail to appreciate that probably in many instances an area of tuberculosis is simply a local manifestation of a more or less general infection by the tubercle bacillus, in an individual whose power of resistance is lowered.

When the attention of a general surgeon is called to the importance of increasing the powers of resistance in an individual patient, as a rule, he acquiesces to the plan that out-of-door treatment should be given patients with surgical tuberculosis, and in an individual case he may adopt this line of treatment. He does not insist, however, on this treatment being given all patients having surgical tuberculosis.

I, therefore, beg your consideration of the means by which surgical tuberculosis can be treated and what modifications in our methods are desirable. The consideration of the subject divides itself as follows: 1, prevalence; 2, diminution; 3, pathological consideration; 4, sanatoriums; 5, construction of sanatoriums; 6, hospitals; 7, out-door treatment of tuberculosis; 8, tent treatment; 9, home treatment; 10, effects of sunlight and electric light; 11, defects of hospital construction.

PREVALENCE.—Tuberculosis in its various manifestations undoubtedly kills more people than any other disease. From one-seventh to one-tenth of the total death-rate of the world is dependent upon tuberculosis. Craig* states that "It has been recently estimated that there are one million cases of active tuberculosis in the United States, and the latest authentic statistics show that 14% of the deaths recorded for ten years are due to some form of tuberculosis."

* Journal of American Medical Association, 1902, p. 229.

Cuzner* states that "Schlenker made 100 consecutive postmortems on adults and children. He carefully examined every part of their bodies and in 65% found tuberculosis. In over 4000 successive postmortems made in Breslau in 1893, one-third of the bodies contained gross tuberculous lesions. If the microscope had been used, probably enough lesions could have been discovered to make 2500 infections. Babes found lesions of the bronchial glands in more than one-half of his postmortems on children. Biggs could demonstrate characteristic lesions in the lungs alone in 60% of his postmortems. Grawitz found primary tuberculous deposits in the lungs in 152 out of 221 cases, this being nearly 70% of all infections.

The most remarkable statistics as to the existence of tuberculosis are those of Naegeli,† drawn from 508 consecutive autopsies done at the Pathological Institute at Zurich. Naegeli did 500 consecutive autopsies and found that 97% of all adults over 18 years of age who died in the Cantonian Hospital were tuberculous. He states that every organ must be carefully examined, grossly and microscopically,

TABLE I.

		Sections.	No T. B.	T. B.	% T. B.	Fatal T. B.	T. B. %	Not fatal T. B.	Not fatal %
67 autopsies from Nov. 4–Dec. 3, 1896.	Under 18	7	5	2	28	1	50	1	50
	Adult	60	15	45	75	21	46⅔	24	53⅓
95 autopsies from Jan. to March, 1897.	Under 18	19	18	1	5	4	100		
	Adult	76	8	68	90	26	38	52	62

Amount of tuberculosis in consecutive autopsies. Naegeli.

* Journal of American Medical Association, Chicago, 1898, vol. xxxi, 1458-63.

† Ueber Haufigkeit, Localisation und Auskeilung der Tuberculose, nach 500 Sectionen des Luricherishen Pathologishen Instituts, von Dr. Med. Otto Naegeli, Arch. Path. Anat., vol. 160.

for tuberculosis. When this was done, it was found that 97% of adults, in a series of 164 cases, had tuberculosis. The first two series of autopsies are appended in Table I.

Naegeli found that the constitution of the individual had no influence as to the presence of tuberculosis. The most powerful gymnast in the prime of his twenty years was no more immune than the feeble, slender shop-girl with flat thorax and so-called phthisical habit. He found that latent tuberculosis was present not alone in those who suffered from chronic affections but in those who died suddenly from wounds of all kinds.

The next two series of autopsies, 346, were even more carefully performed and showed from 97% to 98% of tuberculosis in adults.

TABLE II.

		Sections.	No T. B.	T. B.	% T. B.	Fatal T. B.	T. B. %	Not fatal.	Not fatal %
110 autopsies April–July, 1897.	Under 18	15	12	3	25	3	100		
	Adults	95	3	92	97	21	23.	71	77
236 autopsies July, 1897–March, 1898.	Under 18	47	38	9	24	6	66⅔	3	33⅓
	Adults	189	3	186	98	42	22	146	78

Amount of tuberculosis in consecutive autopsies. Naegeli.

He found that in the first year tuberculosis is exceedingly rare; from one to five years it is still rare but is nearly always fatal; from five to fourteen years it is found in one-third of all bodies examined and causes the death of three-fourths of those it attacks. Reasoning from these statistics, between 14 and 18 years, one-half of the population has been infected with tuberculosis and the process is then always active and but rarely arrested. One-third of all deaths that occur at this period of life are due to tuberculosis.

DIMINUTION. That there has been a great diminution in the number of cases of tuberculosis is readily shown by statistics. Dr. Samuel W. Abbott, Secretary of the State Board of Health of Massachusetts, states* that in New England, with a population of 5,103,187 for the five years, 1893–1897, there were 50,027 deaths, a rate of 19.70 per 10,000. The decrease in the last half century is remarkable. In 1853 the death-rate from pulmonary tuberculosis in Massachusetts was 42 per 10,000 inhabitants; in 1895 the death-rate was 21.8 per 10,000 inhabitants.

TABLE III.

For every 10,000 living there died in the following years:

	TUBERCULOSIS.		PHTHISIS.					
	Prussia	Bavaria	Saxony	Baden	Hessen	Hamburg	Alsace Lorraine	Austria
1876	31		23.6	31.6		31.2		
1877	32		25	30.4	28.5	33.		
1878	32.5		26.	29.4	27.2	33.7		
1879	32.5		25.8	31.5	27.6	33.5		
1880	31.1		25.8	28.3	26.9	31.5		
1881	30.9		23.8	28.6	26.1	31.5		38.5
1882	30.9		23.6	30.7	27.1	30.9		38.1
1883	31.8		24.6	31.2	28.2	33.1		40.4
1884	31.		25.1	29.9	26.3	34.7		39.4
1885	30.8		25.	32.2	29.2	32.6		40.7
1886	31.1		24.9	30.5	29.2	30.8	30.4	40.1
1887	29.3	33.4	23.	27.8	27.	30.9	29.8	37.2
1888	28.9	31.5	23.5	29.6	29.	30.9	30.9	38.6
1889	28.	33.5	23.8	27.7	28.3	26.8	28.7	36.2
1890	28.1		24.2	29.2	30.1	26.2	28.9	39.7
1891	26.7	32.9	21.6	27.1	27.1	26.5	27.9	36.6
1892	25.	31.3	20.8	26.	24.1	25.3	25.4	36.9
1893	25.	31.5	21.5	27.1	27.	24.	24.8	35.4
1894	23.9	31.6	21.4	28.2	26.	20.8	25.7	37.3
1895	23.3	31.1	21.0	26.7	25.8	21.	23.4	
1896	22.1	29.4	20.1	26.5	24.3	20.4	22.3	
1897	21.8	30.	19.8	25.5		20.1		

* The Past and Present Condition of Public Hygiene, p. 67.

Table III, taken from Nothnagel,* shows the mortality from tuberculosis and phthisis in continental countries from 1876 to 1897, shows the diminishing prevalence.

It is naturally interesting to consider the causes that have diminished the frequency of tuberculosis. Dr. Alfred Hiller, in a paper on the "Extinction of Tuberculosis,"† gives a striking chart showing that at the present rate of decrease in the deaths from pulmonary tuberculosis, Prussia will reach the zero point about the year 1927, whereas in England at that time there will be about eight deaths per 10,000, and the total extinction will take place only about twenty years later. The rapid fall in the Prussian rate is ascribed to (1) the precautions against infectious diseases, due to the discovery of the tubercle bacillus; (2) the improved conditions of the working classes caused by the Workmen's State Insurance Laws; (3) the establishment of sanatoriums.

Table IV‡ gives the mortality of 662 villages in France from phthisis and other forms of tuberculosis in 1891. This presents a general idea of the relative frequency of phthisis to other tuberculous affections, and it demonstrates the large number of cases of tuberculosis that occur in a large city like Paris, when compared with a village of 5000 inhabitants. The number of cases to the thousand inhabitants in Paris that occurred was 4.9%. The number of cases to the thousand inhabitants in places less than 5000 inhabitants was 1.81%.

The greater danger of urban life is exemplified by this statement of Dr. H. M. Biggs of the New York Health Board that "One block on Cherry street was inspected on Jan. 1, 1895, and contained 1000 tenants. One hundred and four cases of tuberculosis have been reported in that block in four years and nine months. In a block on Pell

* Special Pathology, Vol. xiv, p. 472.
† English Journal of Public Health, of March, 1903.
‡ La Tuberculose et son Bacille, par I. Strauss, Paris, Rueff et Cie, 1895, p. 482.

and Mott streets, containing a population of 2000, 318 cases occurred in four and a half years.

TABLE IV.

		Exact Figures.			Number proportional to 1000 inhabitants.		
	Population.	Phthisis.	Other T. B. affections.	Total.	Phthisis.	Other T. B. affections.	Total.
Paris.....	2424705	10287	1608	11895	4.24	0.66	4.9
11 cities having from 430000 to 100000 inhabitants	2143380	6262	1535	7797	2.92	.71	3.63
46 cities having from 100000 to 30000 inhabitants	2361244	5474	1764	7238	2.31	.74	3.05
50 villages having from 30000 to 20000 inhabitants	1220019	2595	935	3530	2.12	.76	2.88
127 villages having from 20000 to 10000 inhabitants	1799443	3682	1219	4901	2.04	.67	2.72
332 villages having from 10000 to 5000 inhabitants	2274757	3773	1164	4937	1.65	.51	2.16
95 places having less than 50000.	330802	493	107	600	1.49	.32	1.81
	12554350	32566	8332	40898	2.59	.66	3.25

Mortality from phthisis and other forms of tuberculosis. Strauss.

The relative frequency of surgical tuberculosis is difficult to determine because the mortality statistics do not, as a rule, record surgical tuberculosis. Table V. gives one an im-

pression as to the relative frequency of tuberculous affections other than phthisis. The tables show the mortality at Hamburg* in 1872–1898.

TABLE V.

MORTALITY IN HAMBURG FROM PHTHISIS, FROM 1872–1898.

Age	Less than 1 yr.	1 to 5	5 to 15	15 to 20	20 to 25	25 to 35	35 to 50	50 to 70	Over 70 yrs.	Total.
Male	534	1240	791	1113	2081	5922	7968	4278	350	24277
Female	385	1169	1230	1071	1518	3956	4160	2255	345	16089

MORTALITY IN HAMBURG FROM TUBERCULOSIS OF OTHER ORGANS, 1872–1898.

Age	Less than 1 yr.	1 to 5	5 to 15	15 to 20	20 to 25	25 to 35	35 to 50	50 to 70	Over 70 yrs.	Total.
Male	1033	1764	696	131	116	221	301	243	42	4547
Female	788	1427	659	123	87	157	180	177	43	3641

Leonte,† states that surgical tuberculosis is most prevalent from ten to thirty years of age.

His conclusions are drawn from the following series of cases: From 1896 to 1899 there were 1393 cases treated in the hospitals of Roumania; from 1895 to 1899, 2949 cases were treated in the hospitals of Ephorie; from 1898 to 1900 there were 149 cases treated in Leonte's hospital service at Brâncoveassu. A large number of these cases of surgical tuberculosis were of the bones and joints.

Summary. From the above statistics I think it is fair to state that from 80% to 90% of men and women who reach the age of 40 years have had some form of tuberculosis. This statement, if true, is significant and illuminating.

* Zeitschrift fur Tuberculose, 1900, p. 329.

† Ueber die Chirurgische Tuberculose. Buletinal Asociatunei Medicolor, 1901, p. 212. Zeit. fur Tuberculose und Heilstatteunssen, p. 169, March, 1902.

It should give us the point of view that in dealing with tuberculosis we have a universal enemy, but that, after all, the majority of us are going to conquer this enemy. Tuberculosis is often spoken of as malignant in character, yet this can hardly be true, for if it were malignant in the majority of individuals the human race would be swept from the face of the earth.

PATHOLOGICAL CONSIDERATIONS.—The knowledge of the behavior of the tissues when infected with tubercle bacilli will be of assistance in defining the treatment that should be given an individual case.

The following brief account of the pathology of the disease is abstracted from the syllabus of pathology by Drs. Councilman and Mallory:—The organism enters through the respiratory tract or alimentary canal. It may enter also through the skin, through wounds, in which case a local lesion is produced. The extent and character of the lesion depends upon the number of bacilli, their virulence, the susceptibility of the individual, and upon the mode of infection and the anatomical character of the organ infected. The usual effect produced by tubercle bacilli is proliferation of cells, leading to the production of large epithelioid cells. The tubercle bacilli lie in and between the cells. (Fig. I.) Later, other cells appear, the most conspicuous of which are the giant cells. Miliary tubercles are formed of epithelioid and giant cells with or without a reticulum and are small circumscribed masses varying from .1 mm. to 1 mm. in size. Miliary tubercles undergo degenerative changes, but they do not tend to increase in size by peripheral growth. A nodule is formed by the continuous formation of miliary tubercles in the perpiphery. With the cell proliferation which is dependent upon the presence of tubercle bacilli there is more or less exudation, and this forms a part of the tuberculous area. A miliary tubercle does not tend to enlarge indefinitely, chiefly because there

is gradually formed around it a tissue which is relatively resistant to the action of the bacilli. Such a tissue is also formed around the large conglomerate tubercles. This tissue is converted into dense, cicatricial tissue resistant to the action of bacilli and containing few blood vessels or spaces by means of which the bacilli can pass through the encircling wall. This is peculiarly interesting, for it may plainly be seen that a great effort is made by nature to encompass and control the attack of the bacillus. It is not known how long tubercle bacilli can live enclosed in such a mass. Occasionally the barrier may be penetrated and the old focus form the starting point of a fresh infection. The most rapidly fatal forms of tuberculosis are those in which the exudative (Fig. II.) processes are the chief lesions. The mass formed by the caseation of tuberculous tissue and exudation may remain in an unchanged condition. If composed chiefly of cells it is soft and easily broken up. It is firmer if connective tissue has entered into its formation. It finally may become calcified. (Fig. III.) A much more important process is that of softening. By this the comparatively firm caseous mass becomes changed into a soft or semi-fluid material. This softened material may be carried into the surrounding tissues and general infection may occur. Accepting that this is the general behavior of a tuberculous infection, whether it be in the lung or in the intestine, it is apparent that nature is doing all she can to combat the infection and that we should simply supplement nature in her efforts. A tuberculous process in a lung and in a joint differ largely because of the anatomical differences in the tissues invaded, but the same general principles of infection and repair maintain.

How can we assist nature in her effort to cure the ravages of the tubercle bacillus? She herself surrounds the infection as soon as may be by a wall of dense fibrous tissue. It seems to me that physiological rest to the involved struc-

ture, and improving the powers of resistance of the individual, are clearly the lines along which we should render assistance. This leads naturally to the treatment of tuberculosis in sanatoriums, out of doors and in hospitals.

SANATORIUMS.—The great value of sanatoriums is not alone the control of the infectious patient but their educational influence on the tuberculous patient. Patients are taught a proper conduct of life, taught to protect their fellow-beings from infection, and they are placed, in many instances, on the road to permanent recovery.

The principles of sanatorium treatment in general are well laid down by Dr. T. N. Kelynack.* He states that in selecting a suitable case for sanatorium treatment it is well to remember what hygienic management seeks to secure: (1) The removal of the patient from fresh invasion by the tubercle bacillus, and separation, as far as possible, from all influences aiding in its introduction. (2) The establishment in the patient of processes of repair and the development of the highest power of resistance. To this end the instruments employed are:—(1) Continuous exposure to fresh air. (2) Free access to sunlight. (3) Regulated rest. (4) Controlled exercise. (5) Abundant feeding. (6) Obedience to hygienic requirements. (7) Strict medical supervision. The life in a sanatorium is well described in an article by John H. Lowman ("The Conflict with Tuberculosis," Cleveland Medical Journal, October and November, 1902). He states that the food in a private hospital is such as one would find in a good, well-managed hotel. The patients are allowed to eat as much as they please, but are not forced. As the patient gets stronger he is allowed more exercise. Patients, as a rule, sleep well. The windows are widely opened at night, except in the coldest weather, when the transom is opened. The life in a sanatorium is monotonous, but not necessarily

* Medical Record, Nov. 18, 1902, p. 736.

irksome. Amusements are discouraged; cards and casino life are not permitted. Even chapel exercises and church-going are not approved. A visit to the neighboring village is apt to be followed by a cold, which is in reality an infection caught from association with outside people.

In England, sanatoriums have existed for many years and they are regarded as places for the cure of tuberculosis, not as the home of incurable cases. In France, there are a few sanatoriums principally devoted to the care of children. Germany one finds to be the home of the sanatorium.

The greatest credit is due to such men as Brehmer, who first started the sanatorium at Goerbersdorf in 1856.* In this country we owe a debt of gratitude to Trudeau, of Saranac Lake, who has demonstrated in such a public-spirited manner the value of sanatoriums.

The Imperial Health Office at Berlin* states there are at present fifty-seven public sanatoriums for the tuberculous in Germany, of which thirty-four are located in Prussia. The Imperial German Government, the governments of the different states, the executive authorities, the national insurance institutions, and the municipal governments, are seriously and actively participating in the work of combating tuberculosis.

Germany is the best equipped country in the world for the treatment of tuberculosis, and its statistics show correspondingly favorable results. Hammer† states that in Germany 20,000 people can be given yearly a three months' treatment in her institutions.

The relation of the insurance companies in Germany to the control of tuberculosis is very interesting, and it is hoped that action may be taken by some of the insurance companies in this country towards controlling tuberculosis. The Old Age and Invalid pension laws of Germany

* American Medicine, March 21, 1903, p. 440.
† Munschen Med. Woch., 1902, 49, 1081-85.

(Lowman *ibid*) which were formulated by Bismarck are very suggestive in their scope of action. Under this law every workman and domestic must be insured by his employer against incapacity for work from old age or invalidism and against sickness and accident. Under this law are a number of sick benefit insurance societies, and their reserved fund is $175,000,000. From this fund the sanatoriums are sustained. By an amendment to this law, passed by the Reichstag and called paragraph 45, the insurance societies are permitted to withhold the weekly money benefit from a beneficiary and give it to a sanatorium, and compel the patient to go there to reap the advantage of his insurance.

Frank Warner* states that the United States as a government operates but two sanatoriums, one the Army Hospital and Sanatorium for the treatment of pulmonary tuberculosis at Fort Bayard, New Mexico, and the other the United States Marine Hospital Service Sanatorium at Fort Stanton, New Mexico. One has only to read the reports of these sanatoriums to become convinced that the amount of good accomplished is very great. The New Jersey Legislature has appropriated $300,000 for a sanatorium in which the tuberculous poor may be treated.

Massachusetts has established a State sanatorium at Rutland, but Boston has been singularly negligent in this important public duty. Freudenthal† states that there are 25,000 tuberculous patients in the Borough of Manhattan and recommends the establishing of "farming colonies." He states "We can acquire land enough in this State and in many places near by at a very reasonable price for the purpose of establishing farming colonies. This should be laid out in large farms, and these will fulfill several requirements. Part of the ground should be used for the erection of dwellings for the consumptive colonists and the

* American Medicine, March 28, 1903.
† New York Med. Jour., 1902, p. 146.

rest cultivated. While a large handsome hospital building with all modern improvements looks imposing, it is entirely too expensive for the purpose. I therefore propose to erect tents instead. You can erect many tents for the amount of money that would have to be expended on one building alone. That all hygienic demands can be fulfilled in the best and cheapest way in tent life, every one who has had some experience on that subject will admit." Directly in line of this statement is the statement that there is proposed a sanatorium for the treatment of tuberculosis in New York City with an estimated cost of buildings of $530,000, 480 beds.* This plan provides for a group of structures, the general arrangement of which will be fan-shaped, with the administrative building in the centre and the pavilions projecting like the sticks of a fan, thus giving each pavilion a maximum of light and air. The pavilions are to be two stories in height and connect with each other and the administrative building by covered corridors. Here are two definite plans, one for a sanatorium outside a great city, the other for treatment in tents. The proposed sanatorium to cost $530,000 with 480 beds is too expensive to meet the problem that is presented in the control of tuberculosis.

There is a definite drift of opinion that a change of climate is of less importance in the treatment of tuberculous patients than was supposed, and that it is far more important that the tuberculous should have open air life and a maximum of sunshine, wherever they may live. Kime very sensibly states† that in treating tuberculosis "it is not a question of climate, of elevation, of heat and cold, of dryness or of moisture. Wherever an abundance of God's pure air and sunshine may be had, there may this disease be successfully treated."

* American Medicine, March 21, 1903, p. 444.
† Phila. Med. Jour., 1900, VI, p. 1057.

I give some illustrations showing one of the barracks (Fig. IV.) and one of the resting places (Fig. V.) at the sanatorium at Magdeburg. Figs. VI. and VII. are views of Trudeau's sanatorium at Saranac. Figs. IV., V., VI. and VII. are taken from Zeitschrift für Tuberkulose und Heilstattenusen. Band I, 1900.

In France there are over 20 seaside and mountain sanatoriums, with between 2000 and 3000 beds, mostly for children with tuberculosis. At Ormessau and Villiers the sanatoriums are on a breezy and somewhat unprotected high plateau. The treatment of tuberculous children at the sanatorium at Cap-Breton and at Berck-sur-mer is of a good deal of interest.

Lalanne has charge of the sanatorium at Cap-Breton for the treatment of scrofulous lesions.* Cap-Breton is not really a village but a multitude of little Swiss cottages scattered over the sand near pine forests and close to the ocean. The sanatorium of Cap-Breton is situated upon a dune twenty meters above sea level and 100 meters from the shore. This sanatorium was founded by widow Desjobert, who left 1,200,000 francs to establish a hospital for poor scrofulous children. In this sanatorium there is an abundance of air and light. The climate of Cap-Breton is influenced by the following factors:—(1) the nearness of the ocean, (2) the passage of the Gulf Stream some kilometers from the shore, (3) the proximity of the pine forests. The mean temperatures at Cap-Breton are 13°C in the spring, 22°C in the summer, 14°C in the autumn and 9°C in the winter. In the coldest weather the temperature at Cap-Breton has never fallen below 1°C. Here also there is an abundance of sunlight. The pine forests are far enough away so that they do not affect the action of the sun's rays. There about 250 sunny days in the year. The water is warm enough for bathing from July 1st to October. At

* Bordeaux Theses.

other seasons house baths of warmed sea water are used. The windows of the sanatorium are kept open during the winter nights to permit plenty of fresh air. During the winter days the patients can be out the entire day upon the beach warming themselves in the sun. 71% of patients received at Cap-Breton are cured. Lalanne gives the following typical picture of the improvement of a child:—Upon his arrival at the seashore the child is pale, indolent, sad and morose. He is weak and sickly owing to the bad air of the large cities and the lack of hygienic surroundings. He dislikes to move and nothing interésts him. The appetite (though sometimes good) is often poor, capricious and irregular. A few days after his arrival his appetite improves, but the greatest improvement is to be seen only after some weeks or even months. Then there is a veritable transformation; his condition is stronger, he has the color of health, his muscular powers are doubled, he no longer has that torpor and indolence, he is full of that animation which characterizes children with strong constitutions, he is gay, active, expansive and turbulent. Surgical treatment at Cap-Breton is eminently conservative. They seek to improve the debilitated constitutions and only intervene surgically when it is absolutely necessary. Prolonged treatment at the seashore often suffices to cure many tuberculous manifestations without surgical intervention.

At the sanatorium at Berck-sur-mer, Calot* treats many forms of external tuberculosis. The general treatment of these cases consists of the use of some medicines, cod liver oil, etc., wholesome, abundant food, the observance of good hygiene, the choice of a proper climate and a proper place. Calot believes that sea air is preferable to the air of the country. This probably is a detail. Calot has children even in bed who are taken out daily during the whole winter; the hood of their little carriages pro-

* Bordeaux Theses.

tects them from the rain and snow and large coverings protect them from the cold. The face is uncovered or is half protected with a veil. The cold weather stimulates the appetite and all the tissues of the organism. The temperature rarely becomes so severe as to prevent patients going out. One of the hospitals at Berck was temporarily located 500 meters from the sea for one year and then was moved to the shore. Calot and his assistants noticed that after exposure of the sick to the sea air, the cures of the worst cases became more numerous, more rapid and more complete.

Lannelongue,* Gibney† and Sprengel‡ all recognize the importance of general and outdoor treatment of tuberculous children.

Formiliant§ reports excellent results obtained in the sanatorium on the shores of the Baltic Sea near Windau. Marcou-Mutzner‖ refers to a sanatorium at Hendaye of 200 beds where they receive children from 2 to 15 years of age. The sojourn is from 6 to 9 months. The classes of cases include rickets and anemia but especially cases of initial visceral tuberculosis. The therapeutic agents are the sea air and climate, not extra alimentation and not rest. In one sanatorium it costs but 90 centimes per day to support a patient.

Construction of Sanatoriums.—It is interesting to see the different supplementary plans that are advocated for the treatment of tuberculosis. Let me mention that of Dr. Lederle¶ of the New York Health Department who has sent a letter to Mayor Low urging the appropriation of $35,000 for the establishment of a sanatorium for the treatment of tuberculosis on the tent plan. It is proposed to erect this camp on a tract of 20 acres in Orange

* Gaz. d. Hop. Paris, 1899, lxxii, p. 145.
† Virginia Med. Semi-monthly, Richmond, 188, p. 375.
‡ Berl. Klin. Woch., Dec. 23, 1901.
§ Zeit. fur tuberculose and Heilstatt, July, 1902, p. 366.
‖ Ibid, p. 369.
¶ American Medicine, April 11, 1903, p. 563.

County, which has been offered to the city for two years, rent free. Dr. Lederle states that with the $35,000 he could provide a camp for 60 patients from May 1st to Dec. 31st next. In Germany the following plan has been advocated. Several physicians and a number of associations in Berlin propose to build a large floating sanatorium with roomy, open-air wards, medical supervision, and a complete equipment for scientifically testing the effects of sea air on tuberculosis. The cruising ground is to be the northeastern Atlantic, in the neighborhood of the Canary Islands. Should success attend the effort, other floating sanatoriums will be built. The voyages will last about six weeks.

A most valuable competition occurred recently in England where King Edward VII. offered prizes for the best essay with plans for a sanatorium for tuberculosis in England. One hundred and eighty essays were submitted in competition and the three prize essays were published in the Lancet of January 3, 1903.

The plans of Dr. Latham's essay, Fig. VIII., and Dr. Morland's essay, Fig. IX., appeal to me in that they offer a maximum of sunshine and fresh air.

HOSPITALS.—Hospitals have not, as a rule, recognized the necessity for providing for the out-door treatment of tuberculosis. Dr. F. Rufenacht* states that "in several of the General Hospitals and Infirmaries in England, such as St. Thomas' Hospital in London, the North Staffordshire Infirmary at Stoke upon Trent, the Sheffield Royal Infirmary, etc., consumptive patients have been treated with more or less success by open air methods. In the new Infirmary for Camberwell, the top story in each wing is to be set apart for the the open air treatment of consumptives and is to have nearly all its wall space occupied by windows."

* London Zeit. for Tuberculose, 1901, p. 297.

Burdett* states that "the Bethany Hospital in Berlin has had broad balconies added to the wards where many of the patients are kept in bed, not only in the daytime, but at night." He gives plans and descriptions of the new University Hospital at Halle, which has wide verandas on the south side of the four surgical wards only, the windows being casements, opening down to the floor, so that a bed can be rolled or carried out. He states that the new hospital at Saint-Denis also has verandas on the south side of the wards. The story of the doctors who kept their patients out on the balcony until nine o'clock at night, and finally tried leaving them out all night, at the Bethanien Hospital in Berlin, is interesting. They were entirely convinced by practical experience, by patients sleeping out of doors, of the benefit of this treatment.

Out-door treatment of Tuberculosis.—All of the foregoing leads me to believe that the necessity for out-door treatment of surgical tuberculosis is not sufficiently well recognized by general surgeons.

The improvement that occurs in tuberculous nodes of the neck, without operation, has for years attracted my attention, and in many instances where an operation was inexpedient I have advised the patient to live and sleep out of doors. In the majority of these cases there has been marked improvement and in some cases apparently permanent recovery.

One of the first instances that attracted my attention to the importance of the out-door treatment of surgical tuberculosis was a patient upon whom I had operated for tuberculous peritonitis. The patient, a woman, aged 38, had a typical condition of the peritoneal cavity. She was operated upon, a large amount of fluid was removed, the peritoneal cavity was drained for a fortnight, and the abdominal wound then healed; yet for months the patient did

* Hospitals and Asylums of the World, vol. iv., p. 60.

not improve in the way we expect following an operation for tuberculosis of the peritoneum. During the summer she lived in a tent, and from the time that she lived and slept out of doors her improvement was very marked. During the summer she gained eighteen pounds, all evidence of tuberculosis disappeared and three years later she was apparently in perfect health. At the time this occurred I considered that her improvement was simply a delayed one following operation.

A most striking instance of improvement occurred three years ago. A man, 37 years of age, who was married, came to me for tuberculous epididymitis. The vas deferens on the affected side was corded and knotty. A careful examination of the urine was made and tubercle bacilli were eventually found. Naturally an unfavorable prognosis seemed inevitable. An operation was done and the tuberculous testis and cord were removed. After waiting three or four weeks, the patient having cystitis with tenesmus, a suprapubic cystotomy was done. Two small tuberculous areas, one the size of the little finger-nail, were found at the base of the bladder. Suprapubic drainage was established and I finally advised the patient to go to his home and make himself as comfortable as possible, as a fatal ending was more than probable. The patient was of more than ordinary intelligence and, turning to me, asked if there were nothing else that could be done in the way of general treatment for the disease. He himself suggested to me that if people with pulmonary tuberculosis were benefited by living out of doors, he might be made better by living and sleeping out of doors. I, of course, acquiesced, and he had a tent constructed on his lawn and slept there from April 15th until into December,—out of doors practically every night during the summer. He gained during this time 34 pounds, or from 118 pounds to 152 pounds. At the end of three years apparently the tuberculous pro-

cess has been arrested, he is engaged in his ordinary occupation, that of a book-keeper, and I believe his life has been saved, not by my operation, but by a recognition of the value of out-door treatment of tuberculosis.

In spite of the warnings and protests of experts in the treatment of pulmonary tuberculosis, two errors with regard to the out-door treatment of pulmonary tuberculosis remain deep-rooted and wide-spread in the profession. The first is the sending of patients in advanced stages of tuberculosis to sanatoriums. The second is the failure to recognize the need of absolute rest, literally, sitting still out of doors all day long, on every day, so long as there is evidence that the tuberculous process is still active.

Trudeau has intelligently insisted upon the need of absolute rest during the active period of tuberculosis.

The natural cure of tuberculous areas, it seems to me, demands three things:—(1) physiological rest to the part; (2) pure air and sunlight; (3) ample nutrition to the tissues to check the progress of the process. At the best, the tuberculous area in a lung must "throw out of commission" that part of the lung for the time being. There are not many principles in medicine or surgery, but if there is one in which I thoroughly believe, it is that of physiological rest,* and this principle is just as applicable to the treatment of pulmonary tuberculosis as it is to the treatment of a tuberculous area in the hip- or knee-joint.

Tent Treatment.—Occasionally, here and there within the past few years have appeared suggestions as to the out-door treatment of surgical tuberculosis. Dr. H. P. H. Galloway in 1902,† before the American Orthopedic Association brought forward and advocated strongly the use of tents for the treatment of tuberculous joint-disease. (Fig. X.) Dr. Galloway claims that while out-door life is of

* Hilton's Rest and Pain.

† The general management and constitutional treatment of Tuberculosis of bones and joints. Special reference to the use of Tents.

great assistance in the treatment of all tuberculous bone and joint affections, it is particularly useful in that class of patients whose recuperative power seems to have been exhausted and are being slowly worn out by discharging sinuses which other means of treatment have failed to heal. He does not claim that tents offer the best solution of the problem of securing the nearest approach to continuous out-door life, but that properly designed tents suitably located are much better than the average hospital ward, and that they are an efficient and inexpensive supplement to the regular wards. He thinks that with private patients an effort should be made to secure the benefit of sunshine and fresh air, when for any reason residence in a sanatorium is impracticable or undesirable. Many patients who have a lawn in connection with their homes can be induced to fit up a private tent and occupy it even during the winter months.

For many years the Boston City Hospital, under the administration of Dr. Rowe, has had a tent service in summer, in which, at times, surgical patients have been treated. The advantages of this service have been very marked. Figs. XI. and XII. show the perspective of this out-door camp and an interior view of one of the tent wards.

The means to gain this out-door life for patients are many. An excellent type of tent is described by A. Mansfield Holmes. Figs. XIII., XIV. and XV. show the construction of the tent pavilion. The approximate cost of constructing these tent cottages is $60.00.

Home Treatment.—Dr. Millet, of East Bridgewater, has recognized the necessity for caring for his patients by out-door life, and his article on "The Night-Air of New England in the Treatment of Consumption"‡ is extremely suggestive and very valuable. Dr. Millet has been good enough to let me have illustrations of the "shacks" that he has had built for the treatment of tuberculous patients

‡ Maryland Medical Journal, Jan., 1900.

(Figs. XVI. and XVII.), also of the balconies or verandas where patients sleep, that have been "built out" in connection with many cottages. Figs. XVIII., XIX., XX. and XXI. are balconies built out from buildings in Brockton and Bridgewater, and here the patients sleep, from May until November or December, only the rainy nights driving them in. The improvement in these cases, I am told by Dr. Millet, is very great indeed.

The public are quite aware of the benefits of out-of-door treatment, and Phil May has expressed in Punch, March 4, 1903, the length to which this fad may be carried. (Fig. XXII.)

A. A. Babrow* believes that surgical or local tuberculosis is simply a manifestation of a general infection and may be grouped under two forms, showing certain dissimilarities in their course; the one is a chronic process, characterized by infiltration and formation of new connective tissue; the other is also chronic at times, though oftener subacute, and consists in a cellular infiltration which forms tubercles and through their necrosis leads to cheesy degeneration. In either instance the advent of pus cocci transforms the sluggish process into an acute inflammation. In the absence of a specific remedy our therapeutic efforts must aim at increasing the patient's resisting powers, thus aiding him to overcome the microbic invaders. All hope of salvation by removing the diseased part is distinctly illusory, since surgical tuberculosis must be considered as the local manifestation of a general infection. We combat the chronic non-suppurating variety with local rest and counter-irritating measures, like hot applications, iodin, or the cautery, which all act beneficially by attracting leucocytes and phagocytes to the affected tissues, as well as by stimulating the local circulation. A similar result is obtained by incising and packing with gauze strips, in order to direct

* Russki Vratch, Jan. 18, 1903.

the current of tissue fluids outward. These measures, however, are inadequate when we come to deal with necrotic and suppurating forms of local tuberculosis. Here active interference is called for and operative removal of dead or diseased tissue is advisable. It is to be recognized that such operations are merely palliative measures, as the complete removal of affected areas is impossible in view of the persistent general source of infection. When this truth is recognized, surgeons will no longer be so anxious to operate, but will seek rather to increase the strength of the patient and pave the way for nature's own cure. In this connection climatic influences deserve all recognition. Prolonged stay (one to one and one-half years) at the seashore exerts a marked salutory effect especially in the case of children; the sea air, the bathing, and, not least, the sunlight, contribute their respective share to the final result. Solar rays play a highly important role in the treatment of local tuberculosis and patients should be instructed to expose the diseased parts to the sunlight.

Effects of Sunlight and Electric Light.—In connection with the exposure of wounds to sunlight, it is interesting to consider the use of the Finsen light. Dr. Valdemar Bie,* who is associated with Finsen at Copenhagen, as his assistant, states that Finsen has discovered that sunlight or arc electric light produces a widening of the skin capillaries which may last for five or six months.

"Finsen has demonstrated a widening of the capillaries of the skin which lasts for a long time, as a result of light. Finsen exposed the skin of his arm to an electric arc light; upon the arm were various glass plates and also two letters of the alphabet drawn with India ink; where these covered the skin, the skin retained its white color; the lighted part of the skin, on the contrary, became red, swollen, and later pigmented. After a period of four months the skin was still

* Light Therapy, Deutsche Aerzto Ztg., Berl., 1902, Aug. 15, ii, pp. 361–5.

so pigmented that the white areas were still visible where the glass plates had been; but after five or six months the entire skin was white. If one now rubbed the skin, the marks again became visible, in that the portions of the skin which had been covered with the glass plates did not become so red as the surrounding skin. The dilatation of the vessels of the skin which results from exposure to light can, therefore, last five to six months."

The effect upon the capillaries is only produced where the ultra-violet rays strike the skin. Light produces a stimulating effect upon all the lower animals, and Finsen has proven this upon the embryos of Salamanders and upon worms. He has also shown that the stimulating influence is due to the actinic rays. These rays penetrate cartilage and skin but are absorbed by the blood. This was interestingly proven by Finsen, who found that by rendering the ear bloodless the light would pass through the cartilage and would not pass through the ear when it was filled with blood. The ultra-violet rays have ten times as much bactericidal power as all the rest of the spectrum taken together, but they have very little power of passing through the skin. The chemical rays of light do not reach the deeper parts of the body at all, or at most in a very limited degree. The literature on the use of Finsen's rays is becoming very interesting and one of the best articles is that called Phototherapy, by Professor Niels R. Finsen, Copenhagen; translated by James H. Sequeira, M.D., Lond., M. R. C. P., 1901. There can be little doubt but that the use of Finsen's rays is of definite value in certain cases. Its exact value and limitations have not been determined, but the use of sunlight, it is clear, has been demonstrated from time immemorial. Prof. Poucet allows the sunlight to fall upon a diseased joint two or three hours daily, and Millicz has written a thesis on this subject. Poucet states: "I cannot criticize the results that Millicz describes as good; but

a direct influence upon the tubercle bacilli in this way is not possible; it is hardly to be denied that the nourishment of the limb may possibly be improved by the rich blood supply which results from the light bath."

Robinson* states that the importance of sunlight in tuberculous patients is becoming more and more generally recognized. Abrams† has done some interesting experimentation on the penetrability of the solar rays. He finds that a dry skin is very resistant to the penetration of the solar rays; that the penetrability of the skin is increased by using lanolin externally and subcutaneous injections of distilled water. A very interesting experiment was done, which should be verified. Subcutaneous inoculations with a virulent culture growth of tubercle bacilli were made upon rabbits. The animals were then subjected to the action of the solar rays concentrated at the point of inoculation on ten consecutive days. No tuberculous process resulted, while in other rabbits similarly inoculated but not subjected to sun there resulted tubercles (in which tubercle bacilli were found).

Other experiments were conducted to determine whether the solar rays in greater concentration could be employed by interposing colored glass which, in a measure, excluded the heat rays and allowed unimpeded passage to the chemic rays. The result was an unequivocal demonstration of the fact that when such media were interposed, it was always at the expense of the actinic action on solio-paper. Abrams has used the solar ray in the treatment of a tuberculous ulcer of leg which had resisted the conventional treatment, including rest and curetting, for over a year. The ulcer healed in less than three weeks under treatment with the solar rays. He has also used solar rays quite extensively in aborting furuncles and in the treatment of cervical lymph noditis of presumable tuberculous origin.

* Phila. Med. Journal, 1899, iii, pp. 54–6.
† Phila. Med. Journal, 1899, i, pp. 175–8.

My colleague, Dr. F. S. Watson, has written me a letter of which the following is an abstract. The letter shows clearly that the out of door treatment of surgical tuberculosis is not properly recognized.

DEAR DR. BURRELL:—

It is a remarkable fact that, with the constantly present object lesson afforded by the results of Climatic and Hygienic treatment of pulmonary tuberculosis, when properly and conscientiously carried out, as it is in the best sanatoriums and elsewhere in the hands of some experts individually, so little effort should have been made to apply the same principles to those forms of disease usually classed as surgical. That this is true is indicated by such facts as the following:—

(1) No paper has been presented before the American Surgical Association urging the adoption of that method of treatment for surgical cases, whether in conjunction with or without surgical treatment.

(2) Physicians connected with sanatoriums for the treatment of tuberculous disease, and those practising in the health resorts frequented by persons with phthisis, see but very few patients having surgical forms of the malady. This is illustrated by an inquiry recently undertaken by the writer as to the results of the climatic treatment in that class of cases, in the course of which he addressed a circular letter to every member of the American Climatological Association, which almost all of them were kind enough to answer. The questions were put with regard to tuberculosis of the genito-urinary organs only, and asked the personal experience of those gentlemen as to the results of its climatic treatment. There were just four out of the whole number who did not say that they had no experience in the care of such cases. Of the four, there was one who had had twelve patients of the sort, and the other three reported their observations with regard to from one to four cases, and had not seen more than that number. Some years ago a similar request for information was sent to twenty representative general practitioners, and none of them had knowledge of more than three cases at the most and there were but two or three patients whose history was known definitely for long enough periods to give them any value.

The replies of the members of the American Climatological Association furnished the following information with regard to sixteen cases. In five there was complete arrest of the disease. Seven showed great improvement but still had evidence of the disease. Two were not improved. Two died.

The reply of Dr. Henry Loomis is of interest though it does not supply details. He says, "A number of cases have been sent to the Loomis Sanatorium by genito-urinary specialists for climatic treatment. The results have surprised everybody" (in their favorable character). He refers specifically to one case considered as hopelessly ill by Dr. Keyes, who sent him for that form of treatment, in which the patient was "perfectly well at the end of a year."

It is not necessary to quote from the other letters. I will refer briefly to six cases under my own observation for periods of 18 years in the longest, to ten years in the shortest, in which the patients, though not having been under systematic climatic treatment, have all led out door lives and been in very favorable conditions. Three of them were subjects of renal and three of testicular tuberculosis. In all there has apparently been an entire arrest of the process. In one the disease began in the testes and the seminal vesicles were invaded. Here it has remained without further extension for five years, the patient being in robust health all the while.

This year at the meeting of the American Association of Genito-Urinary Surgeons in Washington, one of the best and most experienced observers among the surgeons of this country, Dr. Chismore of San Francisco, made the first definite and strong plea that has been presented to the Association for a more extended trial of climatic treatment for cases of genito-urinary tuberculosis, as opposed to the surgical treatment.

We are fairly familiar with the results of surgical treatment of some forms of tuberculosis, notably those in which the disease is seated in the bones, which is the most favorable form of tuberculosis which comes under the surgeon's care, and those which involve the genito-urinary tract, in some of which the radical surgical treatment is far from encouraging, to say the least,—nephrectomy for renal tuberculosis, for example, despite the few brilliant successes that have attended it.

What we lack is knowledge of what can be accomplished by the climatic and hygienic treatment of these cases, either in conjunction with surgical treatment or without it, according to circumstances. We can get no information from general practitioners, specialists, or surgeons, for they none of them have had any important experience in the matter, nor shall we have this knowledge supplied until sanatoriums for the treatment of surgical tuberculosis are provided, or else special provision in the present ones is made for the reception of such cases, and are put in the hands of competent surgeons. The medical specialists do not feel competent to assume the care of cases of a surgical character, therefore, surgeons cannot send such cases as need surgical care as well as climatic treatment to resorts which are appropriate otherwise. There are signs of awakening interest, however, in this matter, and it is fairly safe to predict that the pendulum will begin to swing in that direction before long, go to the inevitable too great extreme, and then settle down to its proper excursion.

Very truly yours,

F. S. Watson.

From the foregoing it may be seen that orthopedists and occasionally genito-urinary surgeons have recognized that surgical tuberculosis requires something besides operation. The treatment of tuberculous areas should include not alone active operative measures, when necessary, but the surgeon should always consider the expediency of exposing them to the sun's rays. General surgeons have not recognized its importance.

Defects of Hospital Construction.—A ward for the treatment of surgical tuberculosis should have as its prime essentials a maximum of sunlight and the ability to move the beds out on a veranda, where the patients may sleep, if expedient, day and night. It is perfectly obvious that it is impracticable to construct a hospital at practically $1,000 per bed for the treatment of such a universal scourge as tuberculosis and I now wish to speak of some of the defects that I believe exist in modern hospitals. The failure to

recognize that a hospital must have the maximum of air and light is lamentable.

Recently I had occasion to visit a modern hospital which had been constructed by eminent architects in one of the most intelligent communities in New England. It seemed to me defective in many points. The hospital was beautifully situated but it was winged forwards at the east and west so that much of the south sunlight was shut off. Many of the administration rooms of the hospital were on the south side, which should be devoted entirely to wards and bedrooms. The best part of the hospital building, the top, was occupied by a laundry. The most central part of the buildings where there is the maximum of sunlight, was used as a reception room for visitors. The doors were smooth and made of jointed boards, but the butts projected. The angles and corners of the room were rounded off, but it would require hours each day for a diligent housemaid to clean the carved mantles. The windows were not flush with the wall but were dust-traps. In one small room the best sunlight was excluded by a tree. The conventional shade, lace curtains and picture-molding existed in many of the rooms, but the beds were provided with an aseptic, smooth, painted, steel temperature chart. The windows in the building were small and of such a height that if opened slightly at the bottom the wind would blow directly upon the beds. The walls were not built of hollow brick and already the water of condensation was forming on the walls. The operating room was elaborately fitted with a chandelier directly over the operating table, so arranged that the dust could be readily shaken into an open abdominal cavity. There were gratings over the ventilating shafts instead of cheese cloth. Expensive foot water-taps, which were complex in construction, afforded lodgment for dirt. The wall area in one ward had about one-fifth of the total possible window space. There were no piazzas. Convalescent patients could be placed on

a roof garden which was reached by a ladder. The kitchen was in the northwest top corner of the hospital and the ward maids had the most sunny rooms in the hospital. The engineer of the building slept in a room in the cellar, with a 2x4 window. The operating room had tiled floor, glazed brick, and a marble dado, but the steam pipes were so close to the wall that the room could not be mechanically cleaned.

This construction is going on at the present time in many of our modern hospitals; and it is very natural that it should occur. The Board of Managers tell their superintendent that they want a building to cost a certain amount of money; they employ the best architect they know, a man who may or may not have made a study of hospital construction. The architect and the superintendent of the hospital are placed practically in charge. Rarely is the staff of the hospital more than allowed to look at the plans of the building. The architect naturally turns to the superintendent as the one representing the Board of Managers and what the superintendent wishes is put into effect. Superintendents of hospitals, as a class, are not aware of the advances that are being made in medicine, and of the necessity for air and sunlight in the wards of a hospital. They feel and believe that a monumental structure is necessary, but in consequence of their environment and isolated position, so far as the medical profession is concerned, they do not realize that a new order of hospital construction is or will be imperative. The economies of hospital construction have never, so far as I know, been deliberately studied as have those, for example, of mill construction.

If the State is to deal with the great problem of controlling an almost universal scourge, tuberculosis, a more economical construction of hospital buildings is imperative. This can be done only by architects who are willing and anxious to meet the demand of a minimum cost of construction combined with superintendents who are familiar with, and ap-

preciate the necessity for, the maximum amount of air and sunlight in their buildings.

Prof. T. M. Clark, architect, of Boston, has given me the plans for the building of a proposed sanatorium for Dr. Millet, of Brockton, Mass. (Fig. XXIII.) I find that he has carefully considered the essentials in construction of a hospital for tuberculosis: 1. He has made provision for the maximum of sunlight and air. 2. He has arranged the rooms so that a bed may be wheeled out of doors on a piazza or balcony. 3. He has provided a sun parlor at the top of the building. 4. He has studied the economy of construction. We should recognize that all the south side of a hospital should be devoted entirely to the treatment of patients, and that the north side should be reserved for hospital administration.

Conclusions: The prime essentials for the treatment of tuberculosis are physiological rest, out-door treatment and ample nutrition to the patient. Whether the tuberculous area is in the lung, in a joint, in a lymph node or in the intestines is a detail. Where it is possible, the tuberculous area should be excised, especially if it threatens to invade vital structures; combined with the removal of the tuberculous foci, patients should be treated by physiological rest, fresh air and sunlight. The excision of a small area from the hip-joint, while it may give a brilliant immediate surgical result as to first intention and a cure, mutilates the patient. On the other hand, by the recognition of physiological rest and out-door treatment, such patients may be efficiently treated and life and limb saved.

In the modern construction of hospitals we do not sufficiently recognize the necessity for a maximum of sunlight and air; provision is not made to allow patients in their beds to be moved out on to verandas, that they may be in the air and properly protected.

There is perhaps no community that is so well supplied

with cottage hospitals as New England, and yet how little are they used for the out-door treatment of tuberculous patients.

If the physicians and surgeons of the cottage hospitals in New England will utilize their hospitals for the treatment of tuberculous patients, on the lines of physiological rest and out-door treatment, and will do less major surgery, for example, brain tumors, they will do more good in the community than they do at present. The health of this Commonwealth is largely in the hands of the members of this Society, and if I can impress upon you the necessity, in dealing with this problem of tuberculosis, first to advocate the establishment of an adequate number of sanatoriums for the treatment of tuberculosis, either by the State, cities or towns, a step will have been taken towards its control. Again, as members of The Massachusetts Medical Society, you should recognize that it is not necessary that your patients suffering from tuberculosis should be sent far from their homes, but that by erecting bed platforms, as Dr. Millet has done at the patients' homes, they may be satisfactorily treated in their own homes. The work which is being carried out by Dr. Bowditch at the Sharon Sanatorium is worthy of the highest commendation, but that is for a specially selected class of patients. The work which is done at Rutland must command our admiration, but it is to you, as the practitioners of this Commonwealth, who visit from time to time the homes of the people, that I turn and beg that you will consider the necessity, in tuberculosis, both medical and surgical, of treating patients by physiological rest, and giving them the maximum of air and sunlight.

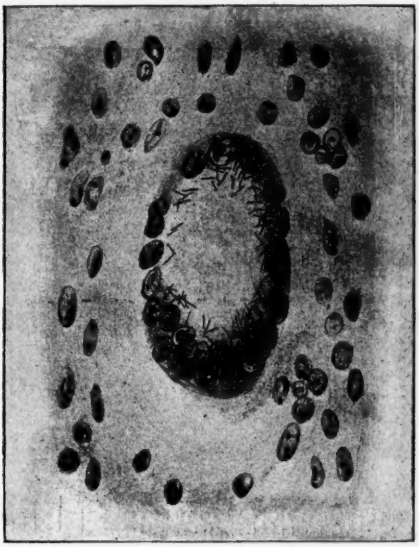


FIG. I.—A giant-cell showing tubercle bacilli between the nuclei in the periphery of the cell.
R. Koch: Ætiologie der Tuberculose. Mitt. aus dem Kaiserlich. Gesund. Berlin, 1884. Band ii.

FIG. II.—Extensive tuberculous process, showing abundant exudate and giant-cells.
(Courtesy of DR. MALLORY.)

FIG. III.—An advanced stage of a healing tuberculous process, with calcification.
(Courtesy of DR. MALLORY.)

FIG. IV.—Barracks at Magdeburg.

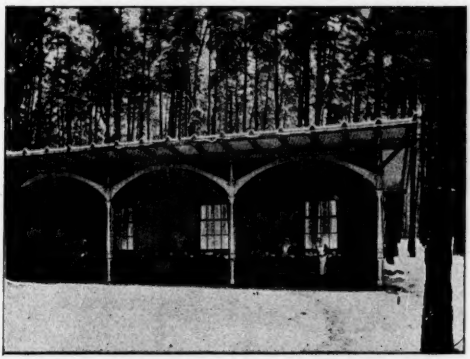


FIG. V.—A resting-place or out-of-doors shelter at Magdeburg.

FIG. VI.—Large, airy, well-lighted recreation-room at Trudeau's Sanatorium at Saranac.

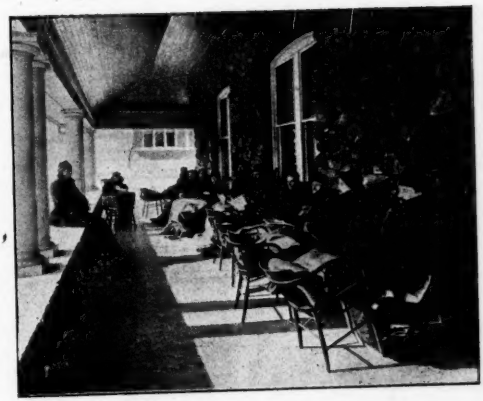


FIG. VII.—Verandah at Saranac in the winter.

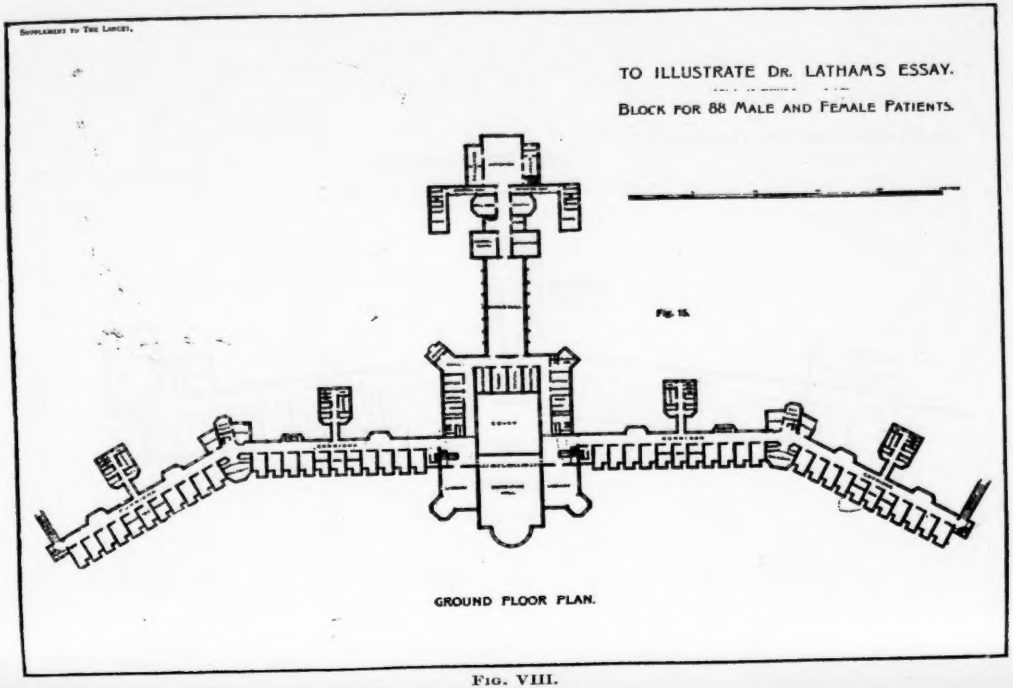
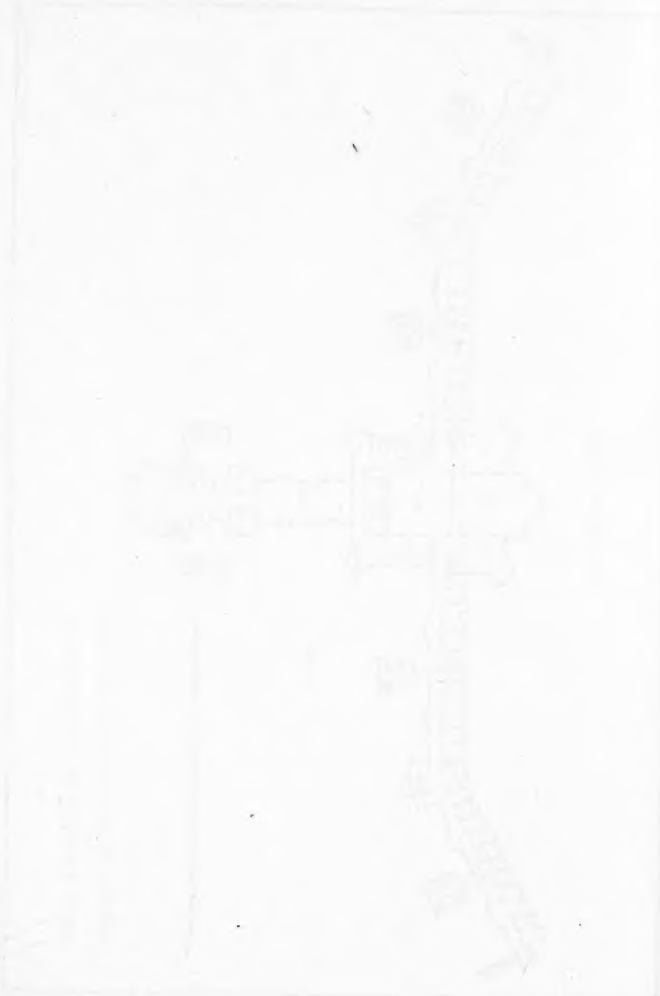

Supplement to The Lancet.
TO ILLUSTRATE Dr. LATHAM'S ESSAY.
Block for 88 Male and Female Patients.
Fig. 15.
GROUND FLOOR PLAN.

Fig. VIII.



TO ILLUSTRATE Dr. MORLAND'S ESSAY.

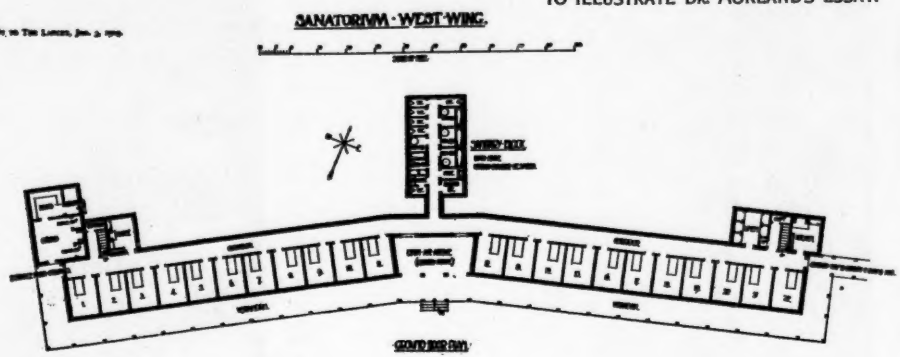


FIG. IX.

FIG. X.—Tents used by Dr. Galloway at the Toronto Orthopedic Hospital.

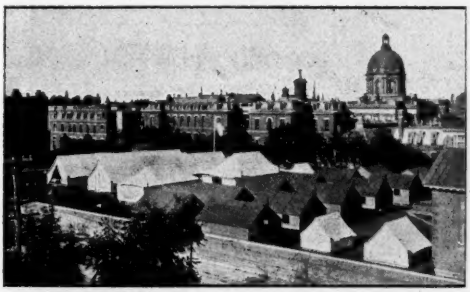


FIG. XI.—Tent-wards. Boston City Hospital.

FIG. XII.—Interior of tent-ward. Boston City Hospital.

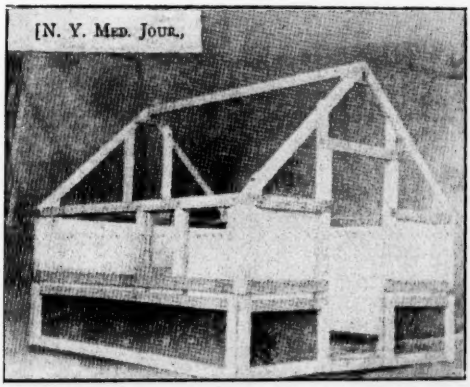


FIG. XIII.—Frame of "tent-cottage"; wainscoting in position. The inner wall of heavy canvas is in position on the front and rear end of right side. The two-inch opening above the wainscoting is clearly shown.

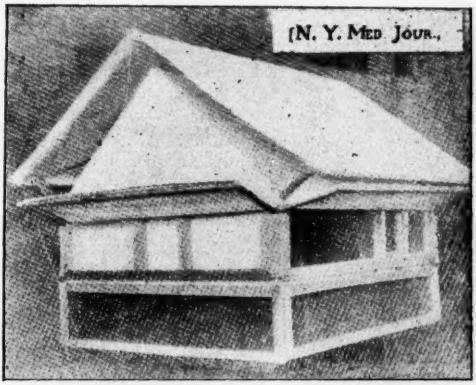


Fig. XIV.—"Tent-cottage" as used in summer. The upper sections of the outer walls are converted into awnings. The rear end shows the inner wall intact. The side shows the upper half of the inner wall lowered, which practically removes the side of the "tent-cottage," converting it into a "tent-pavilion." The same mechanism applies to each side. When thus converted into a "tent-pavilion" the sun can penetrate to any portion of the interior.

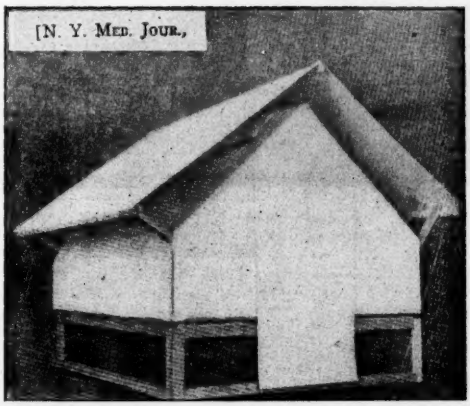
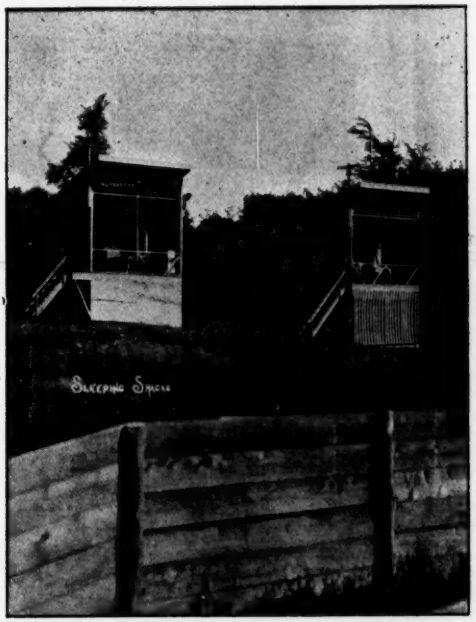


Fig. XV.—"Tent-cottage" as used in winter: the fly intact, the sides and door closed, the roof-window open. The opening between the two sections of the outer wall is plainly shown.



Figs. XVI. and XVII.—"Sleeping-shacks" for tuberculous patients, devised by Dr. Millet. Note the simplicity and cheapness of construction.

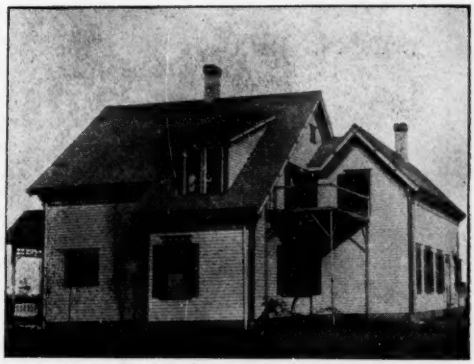


FIG. XVIII.—Sleeping-balcony.

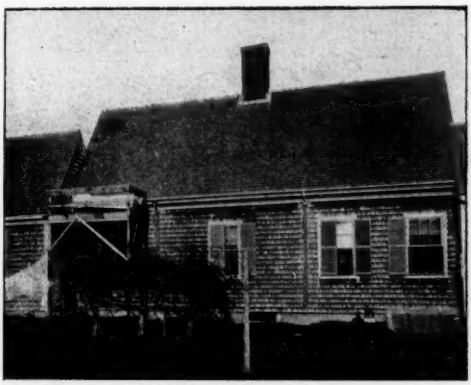


FIG. XIX.—Sleeping-balcony.

FIG. XX.—Roof used as a sleeping-balcony.

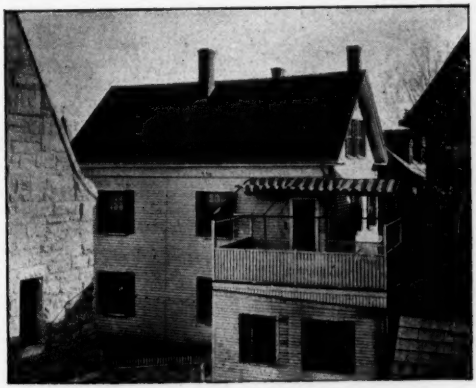


FIG. XXI.—A more pretentious sleeping-balcony.

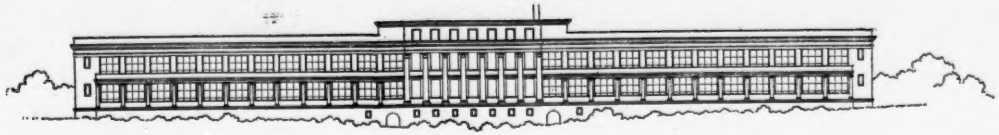
FIG. XXII.

OVERHEARD DURING ONE OF OUR RECENT STORMY DAYS.

"What cheer, Matey! Doin' any Business?"

"Garn! wot yer gettin' at? I ain't 'ere to do Business. I'm takin' the Hopen Hair Treatment!"—PUNCH.

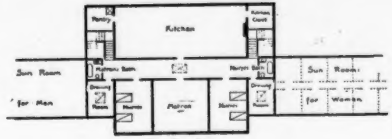
FIG. XXIII.



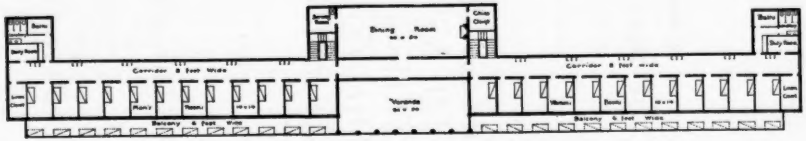
South Elevation.

Basement - Plan

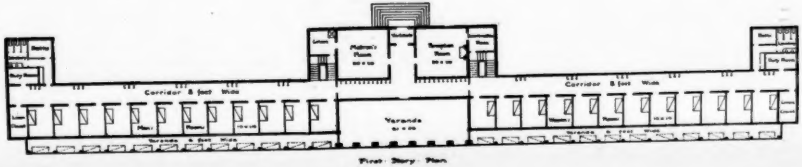
Proposed New Building for the
Millet Sanatorium for Consumptives
East Bridgewater - Mass.
June 4th 1910
T. M. Clark - Architect - 25 Congress St Boston



Third Story Plan



Second Story Plan



First Story Plan

FIG. XXIII.

ARTICLE XXI.

THE SHATTUCK LECTURE.

THE SOURCES, FAVORING CONDITIONS AND PROPHYLAXIS OF MALARIA IN TEMPERATE CLIMATES, WITH SPECIAL REFERENCE TO MASSACHUSETTS.

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INTRODUCTORY.

After I had accepted the honor of addressing you this evening as Shattuck lecturer, several topics of great interest to me presented themselves. After carefully considering them in the light of the provisions of the Shattuck bequest, it seemed to me that a discussion of Malaria, with special reference to our own State, would at this time be the most useful. I accepted this self-imposed task the more willingly as since 1895 I had been giving much time during the summer months, under the auspices of the State Board of Health, to the study of malaria and the local conditions favoring it. Most of the facts I have personally gathered we owe to this Board; for without their active coöperation the topography of the disease could not have been investigated.

If we were to depict the dissemination of malaria, this scourge of an invasive and militant civilization, upon the globe, we would paint it as a black belt girdling the tropical world and extending its lesser shadows in tongues toward the temperate zones. These shadows become less and less distinct the farther north we go. Though the harm it does to northern civilization is infinitesimal compared with its almost forbidding attitude in the tropics, yet even with us the energy needed for the ceaseless struggle for existence

suffers severely when malaria is invading the blood the integrity of which is so essential to our well-being.

Many things have conspired to bring about a renewed public interest in this disease. First and foremost the stimulus created by the colonial expansion of European nations, next the conditions which paved the way for a scientific demonstration of the existence of an intermediate host or carrier in the mosquito, and lastly the tendency of the intelligent classes in our climate toward suburban and rural homes and the general quickening of all interests in animal and plant life, have enlisted not only the attention of the public but their material support for the suppression of conditions favoring malaria. These various interests are perhaps a sufficient excuse for dealing with the newer aspects of this subject on this occasion. But the greater stimulus to me has been the fact that malaria is a prevalent disease of our State, that it has for us a decidedly sanitary as well as economic bearing. Some overworked and apathetic practitioners in rural communities may perhaps say, "What have the newer developments in regard to malaria to do with us? We can cure tertian fever with quinine and we can with equal readiness cure any relapse." This is true as far as it goes; but it would be a very narrow view to take and would, I feel sure, be spurned by our Society as unworthy of its past record and future promise. There is, however, a concealed, or at least overlooked, misinterpretation here which I cannot pass over in silence.

Medicine's legitimate field of work is not only sickness but health. Medical science has been moving the pivotal point of successful combat with disease more and more toward the well man. Hence we have the two great departments of the medicine of to-day, the curative and the preventive. The latter, the latest to develop, is yearly growing in importance and will eventually overshadow the former; for it is based on sounder biological and economic principles.

Preventive medicine acts in an entirely different way from curative medicine. The latter saves its patient here and there and its services are conspicuous and positive, but the former works only collectively in a negative way and its services are inconspicuous. It also saves lives and prevents disease, but only through cold and dry statistics can it lay any claim to its achievements. It works by slow increments. Like the builder's screws which, placed under a building, raise it through an inappreciable distance each day, so preventive medicine slowly raises the sanitary level, and the total aggregate of all the inappreciable increments means many lives saved and much illness prevented. The study of malaria and the application of the principles underlying it mean no new cures, but a steady elevation of the healthfulness of rural life, the only source of a robust generation.

My task was finally made still easier from the observation I have made frequently of late that among many physicians, engineers and educated people generally, the mosquito has as yet no assured position as a transmitter of the malaria parasite. If our recent developments are to have much value, they must be applied at once, the principles must be made operative and enter intimately into the life of rural populations. We have become now fully cognizant of the hurtfulness of sewage out of place and of sewer gas. In fact our legislative sensitiveness to sewer gas became so great that both capital and labor trusts are said to batten on the severe restrictions created by it. Of how much more definite significance in pathology than sewer gas is the malaria organism! But the history of sewer gas legislation shows what can be done if the public interest is once aroused. Fortunately, the prevention of malaria does not demand such artifical measures as are in use against sewer gas, but only such as will add permanently to the utility and beauty of the land.

THE LIFE CYCLE OF THE MALARIA PARASITE HISTORICALLY AND BIOLOGICALLY CONSIDERED, WITH SPECIAL REFERENCE TO PLASMODIUM VIVAX THE PARASITE OF TERTIAN MALARIA.

The proper presentation of my subject demands at the outset a very brief résumé of the main facts of malaria as known to-day, so that we may be able clearly to apprehend the application of these facts in the framing of preventive measures. The development of our positive knowledge of malaria etiology may be divided into two periods. The first embraces the period from 1880 to 1895 and is chiefly concerned with the study of the malaria parasite in human blood and the clinical interpretation of its various appearances. The second period, extending from 1898 to the present, is concerned chiefly with the sexual reproduction of the parasite in the mosquito and with many questions of immunity and epidemiology.

The facts elucidated during the first period may be considered as already common property of the profession. They are included in all text-books on clinical medicine. All students who graduate from first-class medical schools are able to recognize malaria parasites and are prepared to make a diagnosis with the aid of suitable miscrocopes and accessories. Even many older practitioners have prepared themselves to do the same thing.

A detailed account of the discoveries of this first period would thus be superfluous; but I cannot refrain from mentioning the more important stages in its history, if only as a fresh acknowledgment of the arduous labors of those who were the important contributors. For this first period the excellent detailed historical résumé given by Thayer and Hewetson[1] should be consulted by all those interested in the laborious progress of science as well as its immediate achievements. To it I must also refer the student of malaria for a full bibliography to 1895.

In 1880 the parasite of malaria was first recognized as such by Laveran, then a French military surgeon, stationed in Algiers. In 1885 Marchiafava and Celli in Italy pointed out the possible significance of the segmenting bodies as reproductive phases in the life-cycle of the parasite and insisted upon the fact that the parasites were within the corpuscles and not merely attached to them as Laveran maintained. In the same year (1885) a new observer appeared upon the field in Italy whose researches were, next to those of the actual discoverer of the parasite, the most important as bearing upon the parasite in the blood of man. Golgi described in detail the growth of the microöganism of quartan fever and some years later that of tertian fever. He points out differences between these two parasites at various stages in their growth and first definitely associates the segmentation with the paroxysm. He shows that quotidian fevers are due simply to two or three groups of parasites maturing on different days. Henceforth the microscope would enable the skilled observer to make a diagnosis between tertian and quartan fever, to recognize quotidian types and to approximately fix the time of the paroxysm. This great advance did not include in its scope what we now know as the more dreaded æstivo-autumnal or tropical fever, a type of malaria practically unknown in New England, excepting as it may be imported in isolated cases. Councilman was one of the first to insist upon the association of certain forms of parasites with the more irregular and continuous fevers. In 1889 Marchiafava and Celli and Canalis headed a long series of investigations upon this third type of fevers. This is not the time to recount the many shades of opinion, the many and diverse classifications into distinct varieties of the æstivo-autumnal type as the result of laborious investigations. In brief, it may be stated that the tendency is to regard the varieties of this third type of fever as a unity to-day.

With the middle of the last decade the study of the ma-

laria parasite in the human body seems to have exhausted itself. As is the case where the microscope is the only tool, interpretation is difficult and there is always danger in the framing of hypotheses from what we see that we may put the cart before the horse as either method usually satisfies appearances. The stimulus which was to bring renewed life into this subject came from several different directions and with their aid the end the century saw revealed one of the most interesting biological phenomena, in which three different organisms combined to complete what is in many respects a most vicious circle for one of them. The second period presents a congestion of interesting details from several distinct sources. These details are not yet widely known and an historical survey would be misleading. The simplest method which presented itself to me was to give a very concise sketch of the tertian parasite as it is known to us to-day and then refer briefly to the sources contributing the fertile hypotheses.

The parasite of tertian fever in its earliest stages appears within the red corpuscle either as a small ring or as a body consisting chiefly of slender filamentous prolongations or pseudopodia. Examined fresh, it exhibits amœboid movements most active at the temperature of the blood. Soon reddish brown pigment granules begin to appear which increase in number with the size of the parasite. Before the end of forty-eight hours the parasite has grown as large as the host-cell and the latter may be still seen in faint outline or it may have completely disappeared. At this time signs of segmentation appear. The pigment tends to gather into a more or less compact mass in the centre, and the parasite separates into from twelve to twenty-four small nucleated bodies, the so-called spores of the older writers, the merozoites of the present. These small bodies again enter red corpuscles, where the same process is passed through to the stage of segmentation or schizogony. The latter is associated with the paroxysm as already determined by Golgi.

This process of asexual reproduction is the one which is mainly responsible for the symptoms of disease. It destroys red corpuscles and the fever induced may be due either to the corpuscular debris or perhaps more reasonably to the foreign substance of the parasite set free or left behind when the merozoites break away from the mother parasite to begin life anew.

After a certain time, as yet indefinitely known, the process takes another direction. The young merozoites after entering red corpuscles, instead of growing into the segmenting bodies described above, develop into forms which closely resemble those bodies, but which do not segment. They are the male and female elements concerned subsequently in the body of the mosquito with sexual reproduction. The female body is known as macrogamete, the male as microgametocyte. These three individuals, the segmenting body or schizont and the male and female bodies, have been recently distinguished from one another by Bastianelli and Bignami,[2] Grassi,[3] and more in detail by Schaudinn,[4] who gives a number of differential characters pertaining to the structure of the nucleus, the cytoplasm, pigment, vacuole and time of growth of the sexual and asexual individuals. In other words, with proper methods of fixation and staining the skilled observer can determine at an early stage of the intra-cellular parasite to what cycle it belongs.

Leaving aside as somewhat beyond the scope of this lecture the characters of the sexual elements or gametes I may simply state that they develop into bodies which slightly exceed those of the adult schizonts. While the latter rarely exceed 10 mm. according to Schaudinn, the former may become twelve to sixteen mm. in diameter. When they have reached this stage no further changes occur until they have been drawn into the mosquito's stomach. It should be stated, however, that when freshly drawn blood is watched, further changes do occur in the male body, which consist

in the extrusion of spermatozoa or microgametes, the so-called flagella of earlier writers. This extrusion is probably brought about by conditions simulating those in the stomach of the mosquito.

The life cycle of the malaria parasite thus comes to a standstill in the blood of man after a certain time to be completed in another host. In the blood drawn into the mosquito's stomach the two sexual elements or gametes rapidly complete their development. The male element extrudes four to eight flagellate bodies or microgametes which are from 2½ to 3 times the diameter of a red corpuscle in length. These move and lash about actively, tear themselves away from the parent body, and move through the fluid. One of these penetrates into a macrogamete. This process of fertilization is similar to that of higher forms. The male element, composed largely of nuclear matter, unites with the nuclear matter of the female element to form a new nucleus.

The fertilized female body after about ten minutes begins to push out a hyaline prolongation which gradually grows until the entire body has assumed a worm-like form. This little worm (oökinet) begins at once to seek its future abiding place where it is to produce the countless infecting sporozoites. It penetrates the epithelial layer of the stomach and comes to rest beneath the external elastico-muscular tunic of this organ. In this situation, the parasite slowly grows larger and as it grows bulges the external tunic of the stomach, which forms its envelope, outward into the body cavity. Up to five hundred of such bodies may, according to Grassi, develop in the stomach walls of a single individual. Their final size varies more or less according to external temperature and food supply. During the enlargement of these bodies profound internal changes are under way. The fused nucleus of the original oöcyst undergoes repeated division. The new nuclei become the centres

of irregular masses, the sporoblasts. Each new nucleus again subdivides into an indefinite number of minute nuclei, which appear on the periphery of the irregular masses, or sporoblasts, and there by gathering about them cytoplasm, they eventually form the myriads of fusiform bodies known as sporozoites. These when mature become detached from the mother mass, and when the cyst bursts they enter the body cavity. They are very slender bodies, on a average 14 mm. long and between 1 and 2 mm. wide in the middle. They possess the power to change their form as well as to move. After their discharge into the body cavity, some attraction draws them to the salivary glands, which they penetrate and where they imbed themselves in the secreting cells and in the lumen of the gland.

These slender mobile organisms (sporozoites) are discharged into the blood of man when the proboscis of the infected mosquito penetrates the skin and reaches a vessel. Schaudinn recently described the entry of two of these bodies into red corpuscles as observed by him through the microscope. Within the red corpuscle each body begins the life cycle anew.

The time required for the formation of sporozoites in the mosquito varies with the temperature. According to Grassi the shortest period is about ten days. Allowing, according to the same observer, about ten days between inoculation by the mosquito and the appearance of the fever, at least twenty days would elapse after a mosquito had become infected before another case would appear.

We have now sketched, in bare outlines, the life cycle of the malaria parasite and with it the mode of transmission of the disease from man to man. As I stated before, the unraveling of this wonderful chain of events was not accomplished by one man, but a number of contributors have assisted in interpreting this wonderful bit of parasitism.

The germs of hypotheses concerning the mosquito have

been traced well back.* In 1882, A. F. A. King advocated the mosquito theory on the basis of general epidemiological facts.

Manson, in 1894, declared for the mosquito as transmitter. The mosquito draws the parasites into its body with the blood. There spores are formed and when the mosquito dies they are shed broadcast in the air and the water and we become infected when we inhale or ingest them. His hypothesis was evidently suggested by his important work on Filaria.

In the experimental studies upon the malaria of cattle, known popularly as Texas cattle fever, the writer,[5] in association with F. L. Kilborne, after four years of nearly continuous investigation arrived at certain conclusions in 1893 which have been the forerunners of the well-established principles governing human malaria.

The blood parasite in this affection lives within the red corpuscles. The ectoparasite which draws it out of the body is the Southern cattle tick (Boöphilus bovis, Curtice). But the process of transmission is more complicated than that for malaria discovered over five years later. The mother tick which draws the blood and in it the parasite into her body never attacks another animal. After having gorged herself with blood designed for the nourishment of the ova, she drops to the ground, deposits a large number of eggs, and dies. The young larvæ, emerging from the eggs after three or four weeks, attach themselves to cattle and in doing so inoculate the blood parasites transmitted to them by their mother. It will thus be seen that nature had to provide for the infection of an unborn generation of ticks in the body of the mother tick in order to have the malaria parasite transmitted to other cattle. When this complicated mechanism for the transfer of a minute blood parasite came

* For a full historical résumé, including bibliography, we are indebted to George H. F. Nuttall.—Johns Hopkins Hospital Reports, Vol. VIII.

to light after two years of experimentation, it seemed so wonderful that it staggered my belief in the etiological rôle of the bovine malaria parasite, and new experiments had to be tried to make this wonderful mechanism credible. The malarial organism was soon rehabilitated however, when we found that the disease could be produced without the aid of the cattle tick by simply inoculating with the blood of cattle carrying it. It could also be shown that the disease could be produced at a distance by simply importing adult female ticks from an infected locality, breeding the larvæ from the eggs and putting them on susceptible cattle. I speak of these experiments because they completely refute the hypothesis occasionally presented that the mere sting or bite can produce disease by a process akin to poisoning and because they illustrate the symbiosis existing between the cattle and the blood parasite on the one hand and the blood parasite and the cattle tick on the other.

Here we have the first actual demonstration that ectoparasites drawing blood inoculate the virus when they sting. It remained too strange a fact to be properly accepted, but it slowly began to stimulate students of human malaria as shown in their references to the implied analogy. Notably the Italians were impressed with it, and without doubt the fact that Koch[22] in 1898 was able to repeat and confirm the experiment upon cattle in Africa led him to attack the malaria problem with his usual energy. In the mean time Ross[6] working in India had, in 1898, practically worked out in certain mosquitoes the cycle of bird malaria which subsequently proved to be nearly identical with the cycle as observed in human malaria. Ross is therefore the one to whom the credit for demonstrating the malaria cycle belongs.

But there were other contributing sources which were essential to an understanding of the significance of certain forms of the malaria parasite. Since Laveran's discovery in 1880 it was known that certain large forms of the

malaria parasite, when the blood containing them was watched under the microscope, extrude four or more delicate filaments which are actively motile for a time. There were also the peculiar crescent-shaped forms in the blood in the tropical fever which puzzled observers. Both the flagellate and the crescent forms were variously interpreted as degenerations and their significance wholly misunderstood. In 1897, McCallum[7] in the study of bird malaria grasped the true significance of these forms when he saw under the microscope the flagellum of one form penetrate into another.

The sexual process here observed at once gave a new significance to these neglected, misinterpreted flagellate bodies, and to the crescents which we now know as the gametes and which carry on the life of the malaria parasite in the mosquito. At the same time studies by Simond[8] in France on related sporozoa known as coccidia, revealed the presence of peculiar structures which were speedily identified as male sexual bodies. These two converging lines of experimentation, the demonstration of the transmission of malarial disease by the prick of arachnids in cattle and by mosquitoes in malaria, and the disclosure of sexual phenomena in the malaria of birds and in the coccidia, enabled such skilled observers as Grassi in Italy, Koch in Italy and the tropics, Ross in India, and a host of others, to perfect the details of proof and give them weight before the uninitiated and incredulous multitude of professional and lay lookers-on.

There remained still one problem to be disposed of. The universality of the mosquito and the restricted areas of malarial infection are so clearly opposed to one another that the new facts could hardly be expected to make headway without further work. The many species of mosquitoes, rising into the hundreds, cannot all be concerned in this vicious circle. To Grassi more than to any other belongs the credit of having called attention to the genus Anopheles as the

intermediate host of human malaria. So far there seem to have been no developments to contradict Grassi's work. Not only he, but the English malaria expeditions, and quite recently Schüffner,[9] have traced the parasite of human malaria through certain species of Anopheles. At the same time it is not to be inferred that all species of Anopheles are necessarily carriers, as I shall point out later on, and that in any locality the prevailing species should be tested with reference to this point.

It is interesting to note that one kind of bird malaria (*Proteosoma*) seems to be transmitted by certain species of Culex. Of the other widely distributed form of bird malaria (*Halteridium*), the intermediate host remains unknown.

It would be idle for me to assert that with the discovery of the sexual cycle of the human parasite in Anopheles all the vexed questions concerning the epidemiology of malaria had been disposed of. On the contrary, we have just begun to appreciate the sure footing gained and we must now begin to study those elusive variations in the disease due to latitude, climate and seasons, and to racial diversities without which practical measures for the eradication of malaria from any given territory can have but little assurance of ultimate success. It is with special reference to these many variable factors that I venture to present the subject tonight.

THE RELATION OF THE TERTIAN PARASITE TO THOSE OF THE QUARTAN AND THE ÆSTIVO-AUTUMNAL OR TROPICAL FEVER.

It is now well known that there are three species or morphological types of malaria parasites. The tertian and quartan parasites were first distinguished by Golgi, the æstivo-autumnal somewhat later. The distribution of these three forms and the existing controversy whether they are

distinct species or manifestations of but one under different conditions, bring them necessarily within the scope of our subject.

Concerning their distribution it may be briefly stated that the æstivo-autumnal or tropical parasite has its home chiefly in the tropics and extends thence into the adjacent belts of the temperate zones. The quartan parasite has nearly the same distribution, but it is only rarely encountered. The tertian parasite is more cosmopolitan. It is found both in the tropics and in the temperate zones.

A majority of observers are agreed that the three forms are distinct, while a minority are of the opinion that the three forms really represent one species and that a different environment and perhaps different degrees of immunity account for the different forms. This is not the place to enter into a description of the three types or forms. Good descriptions will be found in Thayer and Hewetson[1], Ewing[10], Ruge[11], and others. Suffice it to state that certain differences between them are thoroughly well established. Among them are the crescents (gametes) of æstivo-autumnal fever which are not found in tertian and quartan fevers. The tertian parasite attains a larger size than the quartan and the red corpuscle is swollen and pale. In the segmenting stage twelve to twenty (Thayer), or fifteen to twenty-five (Ruge) spores (merozoites) are set free, whereas in quartan infections only six to twelve spores are formed. There are also differences in the form of the pigment.

The æstivo-autumnal parasite differs from the tertian and quartan not only in the presence of crescents, but also in the absence of sporulating forms in the peripheral blood. The forms there found are rings, at first very minute, then larger. These finally disappear. The segmenting stages are found in the spleen, bone, marrow, brain, etc. These differences, and others not mentioned, upon which observers working in different continents on the whole agree, appear

sufficient to justify the establishment of three species of malaria parasites. Yet there are good reasons for not considering them impregnable. Assuming for the moment that they are independent organisms, we must meet the interesting fact that in our State and in others similarly situated only the tertian parasite is found. In the many specimens from various towns examined microscopically by me and my assistants, the æstivo-autumnal species was not recognized as such in any case. Though many soldiers returning from the Cuban campaign brought this form in their blood, no indigenous cases seem to have occurred as a result of this importation. Recently some evidence has been presented to the effect that the æstivo-autumnal type of fever is gaining a foothold about New York City. Jackson[12] reports four cases at West Point, only one of which was imported from Cuba. Peabody[13] describes the same disease in a man who during eleven years was only once out of New York. He went to Pennsylvania three years ago and was away four days. Patterson[14] thinks that this type began to appear since 1898.* If we maintain that the æstivo-autumnal and the tertian parasites are distinct and independent organisms, we are obliged to support as a corollary one of several propositions. 1. The transmitter of the æstivo-autumnal fever is wanting. This is not improbable even with *A. maculipennis* among us; for the crude external species characters may be the same as those in the South, yet the physiology of the insect may be decidedly modified by a colder climate. 2. The mean temperature with us is too low to successfully mature the tropical parasite in the mosquito. 3. The difference in temperature and other meteorological factors are sufficient to repress and suppress this parasite in the blood while we live in this climate. Some of the soldiers who came from the Cuban war affected with malaria showed at

* I subsequently found that Ewing had reported another case in Journ. Exper. Med., 1902, vi. p. 120.

first both æstivo-autumnal and tertian parasites in their blood. Later on, the former disappeared and the latter remained in some cases producing relapses. This disappearance of the graver type of infection and the persistence of the milder type was noticed by Ewing[10] in the military camp at Montauk, N. Y. Does it mean that the soldiers were infected with two parasites at the outset, or did the æstivo-autumnal assume the tertian form in the northern climate?

The question is of great interest and importance, yet there is singularly enough no decisive argument which can be applied to definitely settle it one way or the other, although the preponderance of observation and experiment is in favor of the pluralistic view. This has been well summarized recently by Ruge.[15]

CONDITIONS UNDER WHICH THE HUMAN BEING MAY BECOME SOURCE AND CARRIER OF MALARIAL INFECTION.

Our present position as regards the etiology of malaria demands at least two links in the chain of causation. If one is absent the disease must fail to appear. They are:

1. Infected human beings.
2. Mosquitoes of the genus Anopheles.

In order to discuss intelligently the relation of human beings to the dissemination of the disease, it will be necessary to recall the fact already described that in the fresh attack the asexual multiplication of the parasite goes on in the blood for a time. Then sexual forms appear which I think are best interpreted as indicating a beginning of immunity. The mosquito can be infected only through the sexual forms. The important question before us is the relative danger as a source of malaria parasites of the infected person who comes from, or lives in, the region usually free from malaria and of one who comes from a region in which malaria is endemic. An attempt to discuss this complex question requires a brief statement of the new doctrine of acquired immunity in malaria.

This was brought distinctly before us by Koch[16] in the reports of his work in the tropics. The idea that immunity can be acquired toward intraglobular parasites was not new, however. It was shown by myself and Kilborne that immunity is acquired toward bovine malaria, and this fact has been widely exploited in our country to prepare cattle for importation into endemic regions where susceptible animals usually die. In investigations made in East Africa and New Guinea, R. Koch came to the conclusion that malaria is very prevalent in young children, but that it gradually disappears with age and that the adults have acquired immunity. In them the enlarged spleen and other concomitants of malaria have disappeared. A. Plehn[17] disputes the latter as not in harmony with facts and cites his own studies of the blood of adults as well as the examination of the spleen to show that they too suffer to a considerable degree with malaria. The point I wish to bring out does not concern their controversy, but their essential agreement upon the great prevalence of malaria in children. Plehn, who made careful observations upon this subject, states that while the youngest children may have numerous parasites in their blood, their general condition remains undisturbed and temperature elevations are uncommon. The same is true of many adults of whom Plehn found about fifty per cent. carrying the malaria parasite without the, to us, familiar clinical disturbances. Christophers and Stevens[18], in their work under the auspices of the Royal Society of England on the west coast of Africa, in India and other regions in the tropics, find that a large per cent. of the native children have parasites in their blood. There is a gradual disappearance with age. After the age of twelve infection is rarely demonstrable. The disease is always mild.*

* "It must be understood that in speaking of a case of malaria in children we are referring only to the presence of parasites; the children are perfectly well and present none of the characteristic signs observed in Europeans affected with malaria." (Lagos, West Coast of Africa.) See Rep. to Malar. Committee. Third Series.

Annett, Dutton and Elliott[19], of the Liverpool School of Tropical Medicine, observed in Nigeria the extensive infection of children. Christophers and Stevens regard this pheomenon of such importance that they base upon the infection of children the prevalence of the disease, or what they call the "endemic index" in any given locality.

Though Koch has been severely attacked for his hastily formed doctrine of immunity, more particularly by Glogner[20] and Kohlbrugge,[21] there can be little doubt that the available data derived from human and comparative sources agree with this doctrine. A distinction should however be made between what I prefer to call clinical immunity and biological immunity. An individual may have acquired a considerable degree of biological immunity, but owing to the effort required and the resulting loss of tissues he may come out of the struggle badly off. This must not shut our eyes to the fact, however. In this protean variation of disease due to the struggle between two variable organisms we must clearly keep in mind "Den ruhenden Pol in der Erscheinungen Flucht" even if it does not always help us cure the patient. The same principle which meets one patient on its downward curve and destroys him, meets another on its upward curve and saves him. We must study the curve and learn where the patient fits it, and where and how to apply assistance.

The general observation in tropical malaria that children are the chief objects of attack, that it is in fact a children's disease there, is another proof of the general doctrine of acquired immunity, for in nearly all endemic infectious diseases the virus attacks by preference the young because immunity protects adults.

The subject is, however, still more complicated. It would be folly to transfer the doctrine of immunity from natives of Africa to natives of our northern climate settling there. The former do not acquire all their immunity in a

single life time. They start at birth with enough inherited accumulation of immunity to need but a little further training to come out victorious. That the Anglo-Saxon baby could do likewise is hardly within the range of probability.

The difference between the native of the tropics and native of the temperate zone is probably dependent on a variety of physiological factors. Among these I am inclined to place differences in the biology of the red corpuscles at the front. The red corpuscle is subject to much greater vicissitudes as regards osmotic changes in the tropics than in temperate zones. With us, temperatures below a certain mean are equalized by clothing and artificial heat. Temperatures above a certain mean are not equalizable. They must be endured. Hence the heat-regulating mechanism must come into more active play, the output and intake of water is large and with it the changes in the concentration of the salts of the blood must vary more or less. The native of hot climates is adapted to these changes, primarily by a greater resisting vigor of the red cells. The temporary resident of the tropics suffers from those disorders incidental to the taking care of corpuscular debris, foremost among which are those of the liver. Added to this natural increase in the resisting power of the red cells is one due to increased use such as takes place when new corpuscles must replace those destroyed by use and by malaria parasites. The blood-forming organs up to a certain point are strengthened by use.

Secondly, a specific immunity due to the malaria parasite itself must accumulate through generations in infected localities, which immunity may reside both in the blood and in the host cells themselves. This subject of the relation of the red cells to immunity I can only touch upon here. It is one which I think will repay thorough investigation.

In the malaria of our latitude, the effect of the parasite upon natives and those coming from still colder climates is

not seen in its frankest manifestations, because quinine enters at once to suppress the disease. When treatment is neglected cases do occur now and then which indicate that malaria freshly introduced into a locality may prove serious and even fatal at times.*

In our climate the French Canadians appear to be most susceptible, and in general it seems to be true, as with yellow fever, that the further north the home of the one who immigrates into an endemic region the more severe the disease when it does appear.

A similar relation between latitude and susceptibility seems to exist in bovine malaria. The disease imported into the northern states from the southern enzootic territory, though carried by healthy animals in their blood, is very murderous to the northern animals. On the other hand, cattle living in the tropics, but not in infected localities, are much more resistant as indicated by Koch's experiments in Africa. The same seems to be true of the Venezuelan form of this disease, called Lomadera, and studied by Ziemann.[23] All that can be claimed from various observations upon immunity is that the influence of the malaria parasite upon the host is in the direction of a biological immunity. The latter may or may not reach a clinical immunity, for that would depend upon the degree of resistance with which the individual started in life, and the interruption of the process by the continued use of quinine.

During the immunization which goes on in the course of the manifest or latent infection, how early do the fully matured gametes appear, and how long do they remain in the blood? After fresh infection does the highly immune body destroy the injected sporozoites or do they form

* In one town a young woman who was being treated without quinine told me that she had had thirty-six chills in thirty-nine days. An examination of the blood certainly confirmed her statement, for the disease had produced marked anæmia and characteristic changes in the red cells.

gametes? These questions are proposed in view of their bearing upon the infectiousness of the individual in different stages of the disease. Schaudinn, who states that with suitable methods gametes of tertian fever may be readily detected in the blood and distinguished from the schizonts or sporulating forms, has seen the former appear in one case after the third attack. But Schaudinn made his studies in a permanently infected region on the eastern coast of the Adriatic in Istria. Thayer and Hewetson have given summaries of their observations of the sexual forms of parasites, and from their report I take the following figures. In one hundred and fifty-three cases of tertian fever, flagellate bodies were seen in six. Of one hundred and five cases of æstivo-autumnal fever they were seen in eighteen. "In an half of the cases the paroxysms had ceased; in eight out of twenty instances quinine had been previously given; that is, for days and sometimes for weeks after the activity of the infection had certainly been overcome, we were able to see flagellate bodies . . . . . ."

In the case of the crescents, the gametes of æstivo-autumnal fever, they found in over seventy per cent. of the cases the crescents appearing in the second week or thereafter, practically none in the first week, if two doubtful cases are thrown out (p. 173). It seems that the gametes tend to appear after the more active process has subsided, and if the conditions where these cases were studied are similar to those in our latitude we may assume that patients may be infectious in the presence of Anopheles during and after the second week. However, I am not inclined to accept fully the findings of the latitude of Baltimore or Istria for Massachusetts.

G. Maurer, studying tropical malaria in Sumatra, states that gametes appear in the blood only some time after the beginning of the fever, in some earlier than in others. In some they do not appear at all. The appearance of cres-

cents is completely suppressed when the earliest fevers are treated rationally with quinine.

There are a number of factors working against the ripening of gametes in the blood in natives of our climate. These are the absence of any inherited or acquired immunity to begin with and the prompt use of quinine. Cases may, however, remain untreated. They may possess more or less natural resistance. Even cases treated successfully in one season may have relapses in the early spring of the following season. Such cases are probably much more likely to prove sources of infection than fresh cases. In them immunity has made some progress. These relapsing cases are probably the starting-point of the infection during the year.

The most dangerous individuals are those who have lived in permanently infected localities for a long time and who possess considerable resistance. In them the virus is latent and prepared to appear clinically under satisfactory provocation. Caccini[25] made an exhaustive study of the latent virus of malaria in Italy and the causes of its occasional outbreak into a paroxysm. He found that the general condition of the people was of considerable moment. The well-fed and well-housed masters had few attacks as compared with the laborers under them. Heavy work during the vintage and harvest, gastro-intestinal troubles, a sudden chill, sleeping out of doors, being caught in rains, all predisposed to relapses. Certain foods causing vomiting, certain medicines, intercurrent illness, even sea-bathing, brought on an attack in some persons. It is related by A. Plehn[26] with citation of cases that persons, having had no clinical disturbance in the tropics during a temporary residence because of prophylactic quinine treatment, on their return to Europe passed through a regular attack either on ship-board or in their native country. In such instances the infection occurred unperceived and may have been repressed by quinine from the start.

Though the conception of the latency of malaria is a very old one, yet its epidemiological significance has had no recognition until lately. The persistence of malaria has been chiefly of clinical interest, and the patient was considered, rather than his environment. To-day we are slowly becoming accustomed to the thought that the latter must also be considered. Absence of illness does not necessarily exclude the presence of infecting organisms and carriers of infection must be kept in view as possible centres of disease. We are now fully acquainted with the possibility that bacteria may persist in the body after recovery. The vexatious question of diphtheria bacilli in recovered cases is present without doubt in the minds of most physicians. In typhoid fever the urine and feces may contain the bacilli after recovery. I might give many additional illustrations. But there is another fact of similar significance, namely the presence of pathogenic bacteria in the well who have come in contact with infection. Thus, typhoid and cholera microbes have been found in the feces and diphtheria bacilli in the throat of well persons not previously diseased. This tendency toward a symbiosis between host and parasite is much more pronounced in the case of animal parasites especially those belonging to the malaria group. Thus in bovine malaria the parasite has been demonstrated by inoculation to exist in the blood of immune animals three years after the last opportunity for infection.[27] More recently Wasielewski[28] has shown that one of the malaria parasites of birds (*Proteosoma*) remained in the blood five months after infection. The more favored theory would undoubtedly be that when the host becomes completely immune, the blood parasites can no longer subsist in him, but I am inclined to believe that such complete immunity does not exist and that whenever an immune human being is infected the parasites go through the process of forming gametes or sexual elements without clinical manifestation and then await the opportunity of being withdrawn

by some mosquito. Or else they await the equally desirable moment when the host has committed some indiscretion by which his resistance is reduced when the parasite rapidly undergoes the usual asexual multiplication in the blood and calls forth a paroxysm. When equilibrium is reestablished the sexual elements appear in large numbers. The forms in which the malaria organisms remain latent is a subject of great biological interest. Grassi assumed that sexual elements or gametes undergo parthenogenesis. Schaudinn recently claims to have established the fact that the gametes may revert in part to forms which multiply asexually in the way familiar to us whenever a relapse is impending.

Whatever may be the biological process, to us its significance is summed up in the possibility of infecting the mosquitoes.*

Without doubt the parasites are so few in the blood in the clinically recovered or the immune cases that only a small per cent. of the attacking Anopheles would infect themselves. The most favorable time for such infection would be at the end of a relapse or a fresh infection when a large number of the parasites would become gametes and thereby capable of continuing the cycle in the mosquito. The attacking mosquitoes would at this stage find plenty of gametes and the danger of creating a focus would be at its maximum. It will be seen that I am putting the burden of starting new foci of malaria and of awakening the disease in the spring upon the older, latent or partly immune cases, who are in that stage of immunity which readily produces gametes after a relapse.

The transmission of sporozoa seems to be accomplished chiefly through the agency of recovered cases. *Coccidium*

* To me the simplest explanation which has suggested itself is that during the period of latency the parasites multiply as they do during the clinical outbreak, but more and more slowly, and in fewer numbers. Each sporulation is drawn out, in time as it were, and leads finally to the formation of gametes.

oviforme of rabbits is handed down from parents to young. This is probably the case with one form of bird malaria due to *Halteridium danilewskyi*. Some years ago I examined in this vicinity the blood of four young crows belonging to the same brood. In one of them the malaria parasites were very numerous. The others were not infected. I examined the blood of the infected bird day after day without seeing any forms indicating multiplication. When a drop of this blood was examined with high powers of the microscope, a considerable number of the largest parasites would rupture the red corpuscle in about two minutes and a half, assume the form of spheres, and throw out a number of flagella. The parasites were all gametes or becoming such.* The infection of the young bird which had not yet left the nest came probably from the parents through some ecto-parasite. In their blood the parasite was probably in a "latent" condition.

The surest way of testing the infectiousness of persons freshly attacked, those who have passed through several seasons of malaria and those who have grown up where malaria is endemic is to determine from what class of cases mosquitoes can infect themselves. It should also be thoroughly ascertained to what extent infection is possible in the latent stage. Experiments made for this purpose by endeavoring to infect mosquitoes have been reported from various countries. The precise history of the cases from whom mosquitoes drew blood is not given and in most instances the experiments were made in localities more or less permanently under the influence of malarial virus. Bignami and Bastianelli refer to a patient with crescents in his blood who successfully infected *A. claviger* and *A. superpictus*. Grassi succeeded in infecting Anopheles from a patient whose blood was full of crescents. W. S. Thayer[29] reported in 1900 a slight infection of *A. maculipennis* after feeding on a case

* The asexual stage of this parasite is not known.

of tertian and one of æstivo-autumnal fever. Woldert[30] writes of one infection after many negative trials with the æstivo-autumnal parasite. Similarly, Berkeley[31] refers to one infection out of sixty dissections. Van der Scheer and Berkelom[32] in an endemic locality in Zealand (Denmark) found that they were able to infect Anopheles when the blood contained gametes, but not when quinine treatment had preceded the experiment. Manson's son was successfully infected with mosquitoes imported from Italy which had fed upon the blood of a case of benign tertian fever.

It is evident that the infection of mosquitoes is not an easy matter and frequently fails. These failures may be in part due to the mosquito, but I am inclined to believe that they are largely due to the patient not being ready to furnish satisfactory gametes. Grassi[3] (p. 115) makes the significant statement that "there are persons who though their blood contains gametes are not capable of infecting Anopheles. These gametes . . . . . are sometimes too young, more often they appear too old, or perhaps the suspected parthogenesis which leads to relapses has already been started." It is quite obvious that the large amount of painstaking work which has been done in studying the transmission of malaria is only the beginning of more detailed and thorough work to be done not only in the tropics but in those localities in our climate where malaria is trying to obtain a foothold, but has not yet fully succeeded.

The persistence of the malaria parasite in the blood explains not only the dissemination of malaria by human beings, but it also throws a strong light upon the many theories formerly held concerning the source of malarial infection. The so-called hydric theory which regarded bad drinking water as a source of infection is quite prevalent in the South. When a person drank bad water, upset his digestive system and had an attack of malaria as a result, the inference that the water introduced the germs was a natural

one. We know, however, that such digestive disturbances simply give the latent virus an opportunity to multiply by temporarily breaking down protective barriers as shown by the exhaustive statistical studies of Caccini. Summing up the evidence concerning human beings as carriers of malarial parasites and as sources of infection, we find that there is no definite information at hand as to the time when affected individuals may infect mosquitoes. Data from various sources including comparative pathology favor the view that human beings are not at once infectious, but that this capacity comes with prolonged parasitism and that it may be retained indefinitely.

MOSQUITOES AS INTERMEDIATE HOSTS OF MALARIA.

To follow the intricate processes of the immunization of man during malaria and their bearing upon infectiousness of the patient can have only a secondary value with us even if it were possible to say at what precise moment the mosquitoes may become infected by drawing blood. The healthy individual even though dangerous should not be restrained if the suppression of malaria can be accomplished in other ways. This leads me to the second link in the etiological chain—the mosquito. The relation of mosquitoes to malaria as carriers became a definite working hypothesis with me in 1896. In 1897 I devised a set of questions for use in the investigation of local epidemics, among which those concerning mosquitoes and the relative exposure of individuals to them were prominent. The hopelessness of making any progress in this subject in a latitude usually free from malaria and only sporadically invaded by it soon became evident. The sequel showed the reasonableness of this state of mind. Not a single fact of fundamental importance in the etiology of malaria has been discovered outside of the endemic foci and the most valuable studies have been made in the heart of the tropics. There are, however, minor prob-

lems pertaining to local conditions which can be solved perhaps better in our latitude than farther south.

Basing ourselves upon the work of Grassi and those who have followed him, we include only the genus Anopheles among the intermediate hosts of the malaria parasite. In our State two species of this genus are widely disseminated and encountered in many localities. These are *A. maculipennis* and *A. punctipennis*. The introduction of these species into Massachusetts is not known. They are, however, not a recent acquisition. Dr. C. S. Minot[34] found larvæ identified by Mr. Burgess as Anopheles in ponds of the Forest Hills district in 1880. Which species they represented is not known. By far the most prevalent and most numerous species is *A. punctipennis*. It is as large as the large Culex species and is particularly conspicuous on account of the yellowish-white spots on the wings and its peculiar position when resting. The adult female, leaving its hiding places in the early spring, measures (excluding proboscis) 5 to 6 mm. in length. The life history differs only in unimportant details from that of the genus Culex. The aquatic larvæ after a period of two or more weeks, depending upon the temperature and food supply, enter the pupa stage. In this they remain about three days and then emerge as sexually mature insects. *A. punctipennis* may be encountered on the wing from March until late in November. In 1902 I encountered a number of females on March 22 when they vigorously attacked me. In 1903 they did the same thing, March 14, a very warm day. Even in mid-winter they may be seen about houses when they have been driven from their hiding places by workmen.

The larvæ, as is now well known, differ from those of Culex in lying horizontally just below the surface of the water with the short air-tubes on the eighth abdominal segment at the surface. They vary more or less in coloration

from a light to a dark green. White dots along the dorsal median line of the abdomen may or may not be present. The largest are about 6 to 7 mm. long. During the summer of 1902 I spent much time in the effort to find some means of distinguishing the larva of *A. punctipennis* from that of *A. maculipennis*, but thus far without success. This may have been in part due to the fact that the latter were quite scarce. The pupæ of the two species of Anopheles differ very slightly from those of Culex. The shorter breathing-tube in Anopheles, as mentioned by Howard[35] has served me well in recognizing them.

A. punctipennis I have found either in the larval or the adult form in different parts of our State. The larvæ were taken to the laboratory and there the development into the adult insect was watched in order to make sure of the species. In this way I have collected or bred *A. punctipennis* from various regions of the Boston district (Forest Hills, Jamaica Plain, West Roxbury near Charles River, Faneuil Station, Cambridge, Belmont), Weston, Concord, Clinton, South Lancaster, Pittsfield and Williamstown. They have been found in Worcester and Springfield. I am inclined to believe they will be found breeding in all parts of our State, if diligently looked for.

A. maculipennis, according to my observations, is a less common species. It is distinguished from Culex with greater difficulty, owing to very slight spotting of the wings. In its position also it differs less from Culex than does *A. punctipennis*, and I have been deceived several times at a distance in confounding it with Culex. I have already referred to the difficulty in distinguishing it from *A. punctipennis* in the larval stage. This species has been collected from Greater Boston. It was found very abundant, in the fall of 1901, near the Charles River in West Roxbury (Parental School). Among specimens sent me from Concord by Dr. Chamberlin, some years ago, this

species was included. Only very few of the insects I have bred in the laboratory were *A. maculipennis*. This may be due to their relative scarcity or to the difficulty of rearing them artificially, as some larvæ usually die. It is of interest to note that in certain localities certain species or groups of species seem to predominate. In the fall of the year when mosquitoes are driven indoors by the cold, in search of places for passing the winter, I have noticed that in one building almost exclusively individuals belonging to the genus Culex would appear. In another building, an eighth of a mile away, individuals of *A. punctipennis* were found almost exclusively. In a third place, three or four miles away, *A. maculipennis* was the predominent species. In a suburb of Harrisburg, Pennsylvania, in August, 1900, Bashore[36] counted 133 *A. maculipennis* and 7 *A. punctipennis*. The conditions favoring the breeding of these species may throw an important light upon the endemicity of malaria in certain places. Thus far they have not been thoroughly studied, and they remain unknown.

It is now generally conceded that all species of Anopheles may become intermediate hosts of the malaria parasites. But some are much more easily infected than others. Of the two prevalent in Massachusetts, *A. maculipennis (A. quadrimaculatus, A. claviger?)* is the one which has been chiefly experimented with in other countries. I am not informed of any experiments with the commoner *A. punctipennis*. Some test of this important matter under conditions and with Anopheles existing in our latitude is very urgent. We should know the relative capacity of the two species for maturing the tertian parasite as drawn from the infected individual. We should know more about the temperature limits within which this development may go on to a successful conclusion. Since such experiments can now be carried on without exposing human beings to infection, by simply dissecting the infected mosquitoes, there is no reason why

this information should not be forthcoming. We are in many respects better situated than those Commissions who have made their investigations in tropical and sub-tropical countries, because we have but one parasite, that of tertian fever, to deal with, while their work has been complicated by the prevalence of three forms.

The application of knowledge gained in tropical countries, while applying in general to our own conditions, may fail in what are biologically speaking minor details, but practically speaking of very great importance. Outwardly and to the entomologist, *A. maculipennis* of our climate and that of Italy or the tropics may be the same.* But there may be important physiological differences due to climatic conditions. Marked differences in susceptibility toward certain diseases may exist in varieties or breeds of higher animals. Similar varietal differences could hardly be detected in mosquitoes by the classifier. Experiments to determine definitely the rôle of our two species of Anopheles in transmitting malaria are called for the more pointedly since voices have been raised recently against the assumption that all Anopheles species act as carriers. Christophers and Stevens found that the very abundant *A. rossii* in and about Calcutta did not convey malaria. When they proceeded in their investigations toward the foothills of the Himalayas, foci of endemic malaria appeared associated with other species of Anopheles shown to carry sporozoites in their salivary glands. Schüffner[9] in studying Anopheles on Sumatra, where malaria prevails chiefly on the coast, came to the conclusion that the coast mosquito soon degenerates in inland regions owing to unfavorable environment.

* The position of the two last segments of the palpi in the male of our *A. maculipennis* is at right angles to the body axis and directed laterally. The figures of the European species, as given by Nuttall and Shipley,[62] Kerschbaumer,[60] Grassi and Celli,[61] have the male palpi nearly straight and nearly in the body axis.

The mosquito, like the higher animals, appears to be the victim, or at least the host of a number of parasites, belonging to widely different genera. In 1901, H. P. Johnson[59] working in my laboratory upon Anopheles caught in the Charles River district, found in about ten per cent. of the individuals of *A. maculipennis* bodies on the stomach wall which simulated very closely the malaria parasite. A very careful study, however, led to the decision that it was some other sporozoon, probably a gregarine.

The breeding places of Anopheles in our State are, as a rule, small, more or less stagnant bodies of water. They do not appear to breed in water kept in receptacles such as rain barrels, tin cans, street traps, obstructed house gutters and the like, but only in water collecting on the ground. But once I saw many Anopheles larvæ in rain-water which had collected in a large iron scoop; but even this was in a country district. This choice of natural bodies of water may account for the fact that Anopheles with us is essentially a rural insect so far as its breeding habits go. Occasionally it has been found in slightly brackish water,[37] but it is essentially an inhabitant of fresh water. It is stated that the insects prefer sunny places, but I have seen them in well shaded brooks.

The food of these larvæ consists of minute forms of life floating in the water, and chiefly algæ and protozoa. The larva during feeding turns its head 180° so that its ventral surface is directed upward. The two brushes begin to swing rapidly. Currents are produced and the minute particles suspended in the water are violently thrown toward the mouth of the larvæ. They are thus dependent upon food floating near the surface layer of water. As is well known, ground-water emerging into the light is rich in those salts which greatly favor the rapid growth of algæ, and E. O. Jordan[38] has found during observations made in Maine that such ground-water contains Anopheles larvæ.

The wide prevalence of these larvæ in the country does not indicate any very great fastidiousness on their part, or any easily determined conditions which must prevail if they are to succeed. Among the places where they are found with us I may mention a few.

In low-lying rich meadows, with very little slope, the ditches used for drainage frequently contain Anopheles larvæ. Where foot-bridges cross them collections of rubbish are likely to form quiet pools for them. In larger brooks, Anopheles larvæ are encountered in pools formed by obstructions in the bed, such as tree stumps, soil carried down during freshets and rubbish thrown into them. Especially where roads cross them, the aqueducts are apt to become dilapidated. Fallen stones and pieces of wood form artificial barriers here to favor the mosquitoes.

Not infrequently small shallow pools of rain or ground-water without any outlet are favorable breeding places. These are frequently the result of insufficient or obstructed drainage due to road building and railroad embankments. The railroads are in fact responsible for many breeding places. Not only has the natural drainage been more or less interfered with by the construction of embankments across drainage valleys, but the soil used for such embankments has often been taken in such a manner as to leave ditches filled with stagnant water on one or both sides.

The larger bodies of water, including ponds, lakes and rivers are not as a rule breeding places of mosquitoes. The enemies of the larvæ seem to predominate in such waters or else the food supply is too limited. Occasionally the shallow margins densely overgrown with water plants may favor them, but even here I have as a rule failed to find any larvæ.

The influence of sewage pollution upon the immunity of the larger bodies of water, more particularly the rivers (*i. e.*, the shallow margins, coves and stagnant reaches)

is an important, though as yet unrecognized, question of public sanitation. It has been my personal experience to find larvæ in several badly polluted streams and not in those comparatively free from such pollution. Whether we are dealing here with a general fact or an accidental coincidence, I am still unprepared to say. Yet there is food for reflection and further study in the suggestion. Water polluted to a certain degree may favor the food of mosquitoes and suppress their enemies. Without going into any detail concerning my observations and impressions on this point, I leave it for future investigation.

Abandoned canals have in some States formed the focus of malaria infection, and there can be little doubt that they are admirably suited as mosquito breeders. Another variety of nuisance is created by brick manufacturing. Excavations are made for clay, and these are abandoned to themselves after the available material has been removed. Some years ago, in trying to account for cases of malaria in Southborough, I found in company with Dr. C. P. Jones a series of depressions overgrown with vegetation and partly filled with standing water. Mosquito larvæ were abundant in some, absent in others. We could not account for these depressions until told that they were old clay pits. A few broken bricks found near them afterward confirmed the statement.

The relation of shallow, stagnant pools and of small brooks and ditches to the mosquito varies considerably with the season and the rainfall. A certain abundance of rainfall may produce breeding places on the one hand, while on the other it may destroy such places already in existence by flooding and washing them out. To take a simple illustration: In 1902 a brook which during the greater part of the summer contained a moderate amount of running water did not harbor any mosquito larvæ. Late in August the amount of water in it decreased, and its bed

consisted of a series of pools with only a little running water. In September I found the stream now containing many larvæ of Anopheles flourishing in the disconnected pools. Here the dry weather favored the mosquitoes. In other parts the dry weather had caused the shrinkage and disappearance of stagnant, undrained pools which earlier in the season contained many larvæ.

Severe drought may favor them by a shrinkage and concentration of water in streams which may greatly favor the food supply of mosquitoes along the banks and in quiet shallow coves. It may also disconnect shallow portions along the banks or shores from the main body and thus create breeding grounds where ordinarily none exist.

High temperature is universally favorable to the mosquito, for its larval life is shortened thereby and the food increased. It is also favorable to the rapid maturing of the malaria parasite in the mosquito. Under a certain temperature the parasite does not ripen.

There still remains one other condition of our surface waters which has been frequently considered the cause or at least the favoring condition of malaria. I refer to the impounding of flowing water artificially by dams. Only those who have explored our State more or less can have an adequate conception of the number of dams which have been built along its water-courses, and which have either been abandoned, or which are still in use. To those familiar with the actual condition of things, an occasional examination of the map is a source of much interest. The streams, large and small, show a series of aneurysmal dilatations which might be considered freaks of nature, were it not that they are the work of man, and that originally beautiful streams have been defaced and deformed to yield the power needed to develop the factory industries of New England.

The site of a dam is usually chosen where its construction

will permanently flood the original banks and form a reservoir. The result is a settling basin in which the suspended matter is deposited, slowly filling up the available space. The next step to offset this filling up is to raise the dam and flood more territory, thereby producing a still broader and usually very shallow basin.

The influence of dams upon the incidence of malaria will depend on a number of factors, and each dam must be considered by itself. The main factor is the relation of the dam to mosquitoes. Among the conditions which may favor the breeding of mosquitoes a few deserve special consideration: —

1. The shallowness of the water and the very gradual slope of the banks which are usually densely overgrown with vegetation and which protect the larvæ from natural enemies.

2. The relative pollution of the water which on the one hand may stimulate the multiplication of microscopic algæ and other food for mosquito larvæ, on the other interfere with the higher animal life.

3. The seasonal fluctuation, whereby in late summer, detached pools and larger collections of water are left as mosquito breeders provided the water in them does not dry up within three or four weeks.

The great danger of such reservoirs has usually been ascribed to daily fluctuations of the water level due to draining off during the working hours and refilling at night. The sloping sides of the reservoirs, formed of a deposit of very fine mud and ooze, are exposed to the sun during the day and in some unknown manner malaria germs were supposed to rise from this alternately submerged and exposed mud. In many instances the original construction of the dam or its subsequent elevation to a higher level flooded woodland and the decaying tree-stumps were looked upon as favoring or incubating malaria germs. To-day we in-

terpret the matter quite differently. There is no evidence, experimental or theoretical, which assigns to malaria parasites life outside of man or the mosquito. They spend their whole life as parasites. Hence the exposed mud flats though offensive cannot any longer be regarded as the *causa peccans*, and the decaying vegetation is concerned only in furnishing food to pools of water harboring larvæ. Dams obstructing the normal current of any stream cannot thus be condemned without investigation. The particular local conditions must be taken into consideration and a careful search made for larvæ at different times of the warm season.

The salutary break in the continued breeding of mosquitoes, and hence in the accumulation of infection, by our severe winters, leads me to consider briefly the ways in which the mosquito escapes destruction during the cold season.

The hibernation or wintering over of adult mosquitoes has been known for many years. They seek the shelter of dark, unoccupied rooms and warm cellars, and the protected nooks in out-houses where they hide in a partly paralyzed condition until the warmth of the approaching spring stimulates them to move and leave their hiding-places. It is surprising to note how many mosquitoes live through the winter in this way. Swarms appear quite suddenly on the first warm day. I have been unable to find among these any males.* The hibernating females are as a rule impregnated and very eager when they have left their hiding places to obtain food. Hence their vicious attacks at this time. The relation of these early attacks upon human beings to the production of malaria I shall refer to again later on. The wintering over of larvæ in water under the ice has been observed in different localities. Its occurrence

* I believe that the testimony is quite general that males have not been found hibernating.

in our latitude has not yet been reported, I believe, and I have no observations bearing upon this subject beyond the one that I have almost always failed to find larvæ late in the fall, shortly before the approach of winter.

The adult females which emerge in the early spring to obtain food for maturing the eggs have not been found with sporozoites in the salivary glands or with sporocysts on the stomach walls. The consensus of opinion seems to be that the malarial infection is not carried over in the mosquito, but that the latter is infected by drawing blood from cases having early relapses or a latent infection.

In concluding this chapter on the mosquito I shall merely mention the hypothesis occasionally brought forward that the infection may pass from the female mosquito to the ova and that the young may spread the germs. This complex process is the one which obtains in bovine malaria, and it is the only way in which the parasite can be transmitted; for the female which draws infected blood never passes to another host. No proof has been adduced in favor of this process in the mosquito; yet I think it should not be lost sight of or cast aside too peremptorily in any future work, for epidemiological studies do not contradict it.*

THE HISTORY AND PRESENT STATUS OF MALARIA IN MASSACHUSETTS IN THE LIGHT OF RECENT BIOLOGICAL DISCOVERIES.

Interest in intermittent fever in our own State has been steadily maintained since the Boylston prize essay of Dr. Holmes. The disease has had more than its share of his-

* A large number of works on the mosquito have recently appeared in various languages. For those intending the more thorough study of species the work of L. O. Howard[93] forms a very good introduction. The largest and most recent monograph is that published in three volumes by F. V. Theobald under the auspices of the British Museum of Natural History. In this volume the reader will find references to other similar monographs. The work of Kerschbaumer[60] gives many interesting details concerning the life history of the mosquito.

torians and chroniclers who have industriously labored to set before us accurate records of its dissemination and prevalence and who have attempted to locate the cause or causes.

As stated by Dr. J. F. A. Adams, there were several distinct epidemics of malaria in New England: (1) the earliest of which we have any records occurring among the settlers, especially in the New Haven region; (2) the epidemic of 1793–99 in Western Mass., (3) that of 1828 to 1836 also in Western Mass., and (4) the one beginning roughly in 1870 and continuing sporadically up to the present.

In Hubbard's "General History of New England," ague and fever is referred to as the "seasoning" through which new comers had to go at New Haven in the earliest years of the Colony. In the second epidemic, toward the end of the eighteenth century, the source of the infection in Mass. is, according to Dr. Holmes's correspondents, Western New York. The types of malaria are spoken of as quotidian, tertian and quartan, and a type is described by Dr. Buel as occurring in the Sheffield epidemic in 1795, whose picture does not fit into the clinical frame of simple tertian. It suggests the æstivo-autumnal fever of more southern latitudes. Yet it may after all have been a very severe type of tertian due to the presence of many generations of parasites. Buel calls it a bilious remittent fever. He emphasizes the pain in the head, back and limbs, the length of the hot fit as from six to forty-eight hours. There was no regularity in the reappearance of the paroxysms. "The fever tended to an intermittent form; but it could neither be called quotidian, tertian, quartan, nor any other name used by authors to distinguish the different species of intermittent fevers." Possibly typhoid fever may have complicated the clinical picture. The immediate occasions for the appearance of the disease were then as now assumed to be the construction of dams, the lowering and raising of the water level in them, and the building of canals. I find mosquitoes mentioned once in

Dr. Holmes's essay as being more numerous than usual durthe epidemic of intermittent and dysentery in the town of Sheffield in 1796.

Concerning the epidemic in 1828 to 1836, Dr. Oliver Peck of Sheffield writes among other things to Dr. Holmes that "the people here generally have a prejudice against the arrest of the disorder, imagining that they must have about a certain number of paroxysms at any rate, and that they are left more healthy if the disease is permitted to go on undisturbed, though bark was formerly, and quinine is at present frequently used, and as I think with benefit. The disorder affects new comers more especially, and more certainly the nearer they reside to the locality." In this letter we have a cropping out of the doctrine of immunity in malaria, already noted in the experience of the earliest settlers who referred to it as the seasoning process. Dr. Holmes's correspondent was as eager then as now to impress upon him that the cases in his town were introduced and not indigenous. Relapses were clearly recognized, and some stated what is not an infrequent occurrence now-a-days, that the recovered patient had a relapse early the following spring. Moreover, the influence of various bodily conditions due to intemperance, exposure and the like in bringing on a paroxysm was emphasized repeatedly.

The reappearance of malaria in more or less epidemic form since 1870 was carefully studied by Dr. J. F. A. Adams. He states that the summers of 1870, 1876 and 1880 were hot and dry, and all streams and ponds became extremely low. These seasons were characterized by an unusual prevalence and rapid spread of intermittent fever; in 1870 in New Haven and vicinity; in 1876 at Hartford and other towns on the Connecticut River in Conn., and in 1880 on the Connecticut River in Mass. and certain localities in Berkshire. The low water of 1880 was extraordinary. The Conn. River had not been so low for many years, and

all artificial reservoirs became drawn down so as to expose an immense area of marshy bottom. The irregular progress of the epidemic is also commented on. It jumped over certain places at first to attack them later on.*

Intermittent fever around New Haven was described by Dr. H. Bronson[40] in 1872. The first case he traces to a saw mill in West Haven in 1863. The epidemic in South Framingham in 1885 has been investigated by Dr. Z. B. Adams[41] and described in a graphic manner. It seems to have been associated more or less directly with the building of a new conduit through Farm Pond.

Dr. C. H. Cook[42] in 1890 published an exhaustive enquiry into the prevalence of intermittent fever in Mass. From one hundred and fifty-two cities and towns responding to his circular letter, eighty-six reported intermittent fever, "an increase of thirty-nine over the number reported in 1880 by Dr. Adams." Thirty-four towns reported sporadic cases only. In fifty-two, the disease was more or less epidemic. In 1894, Dr. J. J. Thomas[43] collated information obtained from physicians and hospital records in towns along the Charles River which showed a very great prevalence of malaria, especially in the town of Newton. The largest number of cases occurred in 1891–4, although in the years 1890 and 1891 the cases had been numerous. There are still two other papers dealing with the same theme whose authors made careful local enquiries. In 1901, Dr. F. L. Morse[45] investigated the prevalence of intermittent fever, past and present, in the towns bordering the Sudbury and Concord Rivers. The earliest case of which he could obtain any definite information occurred in 1880 and is said to have been acquired in Boston. Among 1186 persons, 209 cases occurred during the past twenty years.

* A somewhat similar survey of this last wave of malaria was published by Dr. C. V. Chapin. (The origin and progress of the malarial fever now prevalent in New England. Fiske Fund Prize Dissertation. No. 32. 1884.)

A similar enquiry was made by Dr. Chamberlin[45] for Concord, for the period from 1870 to 1900.*

In addition to these various more or less exhaustive reports we have certain data collected by the State Board of Health in the routine examination of blood films for the malaria parasite. These data are valuable since they are based on the demonstration of the parasite in the red blood corpuscles.

In 1895 the Board began the distribution of coverslips in small metal capsules upon which blood films from suspected cases were to be spread, dried and mailed to the laboratory. There the films were stained and carefully scrutinized for the malaria parasite. Since the establishment of this service many cities of the Commonwealth have created their own diagnostic laboratories, and material from the cities rarely finds its way to the laboratory of the Board. Many physicians do not consider it worth the trouble to send in blood films where the fever is clearly intermittent and is cut short with quinine. In spite of the incompleteness of our records concerning the prevalence of malaria they are quite suggestive. Since April, 1896, malaria parasites have been demonstrated in specimens from twenty-eight different towns. Some of these towns have sent specimens for three consecutive years. Very few appear but once on the list. In all, 175 individual cases were found infected. What proportion of the whole these represent I am unable to state approximately; but I am inclined to believe, from conversations with many physicians practicing in the country, that they represent only a small fraction of the actual total.

Under the most favorable circumstances the study of the distribution and epidemiology of malaria remains difficult for two reasons. The most inveterate malaria carriers know

* See also: Malaria in Boston and Vicinity, by C. W. Townsend. Boston Med. and Surg. Journal, 1892, cxxvii. p. 615, and Intermittent Fever in Brookline, by H. L. Chase. *Ibid*, p. 565.

how to treat themselves with quinine, and thus the very cases which might explain the incidence of intermittent fever escape the enquirer. In the second place the very people who in other matters pertaining to the public welfare may be depended on for aid are often averse to giving information concerning malaria in their neighborhood. Dr. Bronson stated in 1871 that "as a rule men having residences or building lots for sale are, on the ague question, not above deception."

In attempting to interpret the appearance of these waves of intermittent fever we must distinguish between the original sources of the infection and the local favoring conditions. The malaria parasite is not created anew by telluric and meteorological conditions, but it comes from some preexisting parasite. Where did it come from? In the tropics malaria is potentially present at all times. With us it appears as a rule quite unexpectedly and may again disappear. The belief was quite general that upturning of the soil was responsible in some way for the disease. That it closely follows operations involving excavations such as railroad building, grading, tunnelling, and the building of dams, dikes, bridges and reservoirs is well known. The immediate popular inference was the crude, unbiological notion that the germs of malaria live in the soil. While studying the appearance of malaria along the line of construction of the Metropolitan Water System in 1897 and the epidemic in Billerica and Concord several years later, I became firmly impressed with the belief that malarial infection is imported by workmen. This theory* was suggested by experimental studies in bovine malaria which showed that all cattle from endemic regions carry the germs in their blood and that the disease can readily be produced by inoculating a little blood from these healthy, immune animals from infected districts. That workmen from malarial countries may carry the para-

* Was tentatively published by me in 1901 (see lit. 47 and 63).

site in the same way perhaps indefinitely was the only theory that could satisfactorily explain the observed facts. Since that time the theory that human beings are the sources of malarial infection has become firmly intrenched. Koch[16] in his Italian investigations stated that the infected human beings must be considered the starting-point of every season's new cases. Christophers and Stevens reporting to the Royal Society from their field of investigation on the West coast of Africa go so far as to strongly argue segregation of Europeans from the natives, as the latter are largely carriers of the parasites.* The same authors point to the analogy between malaria and Nagana or the Tsè tsè disease. A fly transmits the blood parasite of this disease from apparently healthy wild game and from buffaloes to the domestic animals imported from Europe. The latter usually contract a fatal disease. While admitting the analogy I must confess that that between human and bovine malaria is much closer. The tsè tsè parasite is a flagellate and lives free in the blood. The parasites of the two forms of malaria are sporozoa, both live in the red corpuscles and have many characters in common. Christophers and Stevens, furthermore, ridicule the idea that malaria can be acquired in regions free from human beings and they show that there are very few places even in the heart of Africa where the explorers are not near natives.

When the theory that the laborers and not the upturned soil were responsible for malaria was first formulated in my mind I felt that in so important a matter there should be ample proof. New theories or hypotheses unsupported by facts are more dangerous than none at all when promulgated,

* "These facts are of importance as they show that the generally received idea that Europeans derive malaria from preexisting cases in Europeans requires considerable modification. This factor, it seems to us, sinks into complete insignificance beside that of infection derived from native sources." (Rep. to Malaria Committee. Royal Society, Seventh series.)

since many are contrary to fact and if applied are certain to cause damage. It seems to me that taking into consideration the abundant contributory and demonstrative evidence which has been accumulating since 1897 we are quite safe, in our efforts to suppress malaria, in now taking this theory as a fact and acting upon it.

If then we focus our attention upon man as the carrier of malarial infection, and if at the same time we consider that the more "seasoning" a man has had up to a certain point the more ready he may be to have gametes in his blood after infection from a less seasoned neighbor or bed-fellow, we shall be able to explain many of the peculiar freaks played by malaria in our State.

The experience of the early New Haven settlers in going through a "seasoning" process is the same as that experienced by the early settlers of the middle west. Malaria was quite prevalent during pioneer days, but it slowly declined and in many places disappeared. This decline and disappearance were usually ascribed to the drainage and cultivation of the soil. This was, to be sure, an important factor in suppressing local breeding grounds of mosquitoes, but these did not originate the germs. The latter were, in part, brought by pioneers from endemic regions in the south and transmitted to the susceptible population from the north. A third factor, which in many places and probably also in the Connecticut colonies may have been the predominant one, was the Indian. The aborigines, when exposed to malaria, must have gone through the process of immunization just as to-day the native Africans are going through this process from infancy on. Even if we assume that the adult was wholly immune and free from infection, there must have been all grades of immunity in accordance with age and frequence of exposure in any given locality. With the gradual thinning out of the natives or their forced migration

and the introduction of Peruvian bark, the malaria parasite was gradually destroyed.*

Of the outbreak in western Massachusetts, at the end of the eighteenth century, the sources of infection can be discovered only by a careful study of contemporary local history with special reference to the movement of the population and the modes of life. Even without any appreciable influx of immigrants from malarial countries, our own southern states could have furnished at any time a sufficient quota of malaria carriers to start an epidemic under favorable local conditions. Dr. Bronson, in his quest for the origin of malaria around New Haven, found what he took to be the first case in 1863 on Cove river in West Haven in a sawmill. "While thus occupied (repairing in the pit and about the flume of the mill) in August, 1863, in a season of drought, he [the patient] was seized with a fit of ague. *He had been at the south several years before and knew the disease but had not had it.*† According to his statement, his was the first case in that quarter and the only one that year."

The last invasion of New England by intermittent fever, beginning roughly with 1870 and extending to the present, a period which has been so well described in its various manifestations by others already referred to, was largely due to the great material development, which found expression in many new public and private enterprises, such as the reconstruction of railroad beds and highways, the introduction of public water supply and sewerage plants, the building of mills, and the draining, filling and grading of suburban territory and the like. In connection with these enterprises, the source of malarial infection during the last thirty years I regard as coming from the Italian, Hunga-

* It is of interest to note here that the year of the introduction of Peruvian bark by the Countess of Chinchon, 1638, witnessed the planting of the New Haven colony. Quinine, the active principle, was not used until 1820.

† The italics are mine.

rian and other immigrants from endemic foci, and the contingent of colored laborers from the southern states.

There is a remarkable coincidence between the growth of this foreign body of immigrants and the progress of malaria in New England in these latter decades. Those who have traced its movements claim that it came from the southwest, that is from the New Jersey coast, and spread northward and eastward. I am inclined to believe that it came from the transatlantic steamers landing their passengers in New York city.

The twelfth census gives these figures of the foreign-born population from Italy in our country since 1860:

1860 . . .	10,518	
1870 . . .	17,157	
1880 . . .	44,230	
1890 . . .	182,580	Increase 312.8%
1900 . . .	484,027	Increase 165.1%

Of that same population there were males of voting age, in —

1890	70,853 aliens,
1900	133,810 . "

the total number of males of voting age in 1900 being 252,534. The predominance of men is seen by comparing this figure with the total Italian population given above.* Of this large contingent Mr. Prescott F. Hall[48] states that 95 per cent. are destined for New England, New York, New Jersey and Pennsylvania. What proportion of such immigrants come from malarial districts of Italy it would be impossible to determine with any accuracy. The Commissioner of the Bureau of Labor Statistics of Connecticut

* The annual immigration from Italy was 12,354 in 1880. Fluctuating more or less during the decade, it reached 52,003 in 1890. During the following years it was usually above this figure. In 1900 the total was 101,662, of which 84,346 are reported as from southern Italy. In 1902 the total reached is the enormous sum of 180,535, of which 152,915 are from southern Italy. (From figures kindly furnished by Mr. Prescott F. Hall, Secretary of the Immigration Restriction League.)

is quoted by R. Mayo-Smith[49] as stating that "the Italian immigrants come almost entirely from the southern districts of Italy." In 1897 I interrogated twelve Italians living in a laborers' camp in Southborough. Five claimed to have come from Naples, three from Rome, and only one from Piedmont. Three did not give the district.

Concerning the importation and use of Italian laborers, I quote from Prof. R. Mayo-Smith, the following statement:

"There has been a large number of such cases (laborers imported under contract) during the last ten years (1880–1890). Many of the Italians who have worked on our railroads have been brought by contractors for that purpose. The New York commissioners of labor, in 1885, found a contractor in Buffalo who admitted that he had furnished four hundred foreigners to railroad companies and other corporations during the preceding year. We hear of similar cases in New Jersey, Kansas, Iowa and Wisconsin. The commissioner of the labor bureau in the last named State asserts that in the year 1886 the State was flooded with circulars from an Italian labor and construction company in New York, offering to let men for 'tunnelling, grading, mining, breaking stones, laying ties, repairing washouts, laying water and gas mains,' etc."

Among the other foreign immigrants from southern Europe who may bring the malaria parasite and who are of sufficient importance, numerically, to be considered here, are those from the basin of the Danube and its tributary, the Theiss in Hungary. The twelfth census furnishes us with the following figures of the foreign-born Hungarians:

1870	3,737
1880	11,526
1890	62,435
1900	145,714

Of voting age, there were in —

1890	38,749
1900	72,897

Assuming that these figures are fairly accurate, there were in the United States, in 1900, over three times as many foreign-born Italians as Hungarians.*

Malaria may also be disseminated by chronically infected inhabitants of malarial districts in the south when conditions are favorable, as in camps of negro laborers. A number of cases occurred in 1897 in such a camp near West Berlin, Mass.

The thousands of immigrants from malarial regions are under ordinary circumstances absorbed without causing any appreciable increase in malaria. Those that go into the crowded quarters of our large cities are not exposed to the bites of anopheles, excepting in rare instances. Those that go into the country districts to till the soil are not likely to create a focus of malaria, as the conditions for transmission from isolated cases are not very favorable in our climate. Under ordinary circumstances, therefore, the scattering of such potentially dangerous people over a wide territory would perhaps arouse no attention or apprehension.

There are many examples of imported malaria, known to physicians of our latitude, before whose door the spread of malaria cannot be laid. The returning soldiers of the Civil and the Spanish wars did not, so far as observations go, infect their neighborhood. Sergent[59], after studying the subject in Paris and its suburbs, states that although there are many anopheles in the suburbs, and some chronic cases of malaria in the city from the colonies, no indigenous cases came to the notice of the author.

When, however, a considerable number of individuals remain together, work in rural districts in proximity to breeding places of mosquitoes and sleep at night in sheds

* In 1880 the number of immigrants from Hungary was 4,363; in 1890, 22,062. In 1902 the total Magyar immigration was 23,610. In the intervening years there was the usual fluctuation, corresponding with periods of industrial depression and prosperity.

built in the open country, usually near some small body of water and wholly unprotected from the attack of the omnipresent species of anopheles, the situation is wholly different. Under such conditions, usually, some one or more individuals carry malaria parasites. These are transmitted to the non-immune or only partially immune fellow-laborers. Opportunity for the infection of mosquitoes with gametes from the blood is ample; and, sooner or later, the infected mosquitoes find their way to the very susceptible natives of the territory where the work is going on.

We are, all of us, tolerably familiar with the condition I have described. It is a method employed many years ago in pushing railroad construction forward in more or less unoccupied territory where it was necessary to retain the force of men near the scene of operations. This method of carrying on work in more or less densely populated districts has been in use certainly since 1870. Dr. Bronson furnishes the following illustration for the New Haven district, which I quote verbally:

"In pursuance of this agreement they drew down the water, and when the ground was sufficiently dry, in August, 1871, set about forty men to work cutting the bushes at Pine swamp, and in a tract of about twenty acres at the head of the lake. The men were occupied some four weeks cutting and burning, during which time about half the party slept on the ground near their work. Of the whole number, only one had the fever before the job was completed, but I have heard of two or three others who had it afterward."

Dr. J. F. A. Adams quotes the following letter received from Dr. C. Seymour of Northampton, Sept. 27, 1880:

"A very large percentage of the cases have occurred at a railroad station on the Connecticut river, two miles below our village. Here is located a large sawmill, employing seventy-five men; and these have quite generally suffered. Many of them have never consulted a physician, as they know quinine cures the 'shakes,' and get and use it accordingly."

Dr. Z. B. Adams says that in 1885 "an Italian, a laborer upon the new conduit through Farm Pond, was sent away together with several companions suffering from the fever."

I have the microscopic records of malarial infection from several laborers' camps. Such cases were usually overlooked some years ago. If discovered they were supposed to be a product of the environment, the upturned soil, the swamp or the partly drained pond or stream about which they were at work. Many patients treated themselves. The presence of malaria was recognized only when the infection had become disseminated and had attacked the inhabitants living near by.

Occasionally the accumulation of infection goes on so rapidly that veritable epidemics are the result. The disease appears in an explosive manner like typhoid fever or cholera. A good illustration of this type of outbreak occurred in Rhode Island in 1880[51].

On August 18 a workman in the yards of the Nayatt Brick Company was taken with chills and fever. Five days later sixty workmen were affected, some with very severe symptoms. On August 25 the disease reached its climax with seventy on the sick list. About Sept. 1 there was a general lessening. On Sept. 20 nearly fifty were again on the sick list. During the season one hundred and eighty-five persons, comprising almost the entire force, had been affected. Of the workmen attacked nearly all were Canadians. They slept in two lodges near the works. The two upper stories were occupied as sleeping rooms, the highest being twenty feet from the ground. Both floors were equally affected. The epidemic came very unexpectedly for cases of malaria in this vicinity could not be recalled by the oldest inhabitant.*

Another epidemic of a different character as regards time

* Similar epidemics were noticed by Dr. E. H. Stevens[52] in the Cambridge brickyards among French Canadians in 1892 and previously.

and topography is the celebrated New Lenox case described by Dr. J. F. A. Adams[39b].

In 1870 the dam of the Smith Paper Company on the Housatonic river was raised three feet above the already existing four feet. In the lower half of the district influenced by this dam, which caused a setback of four miles in a direct line, water spread out irregularly to an average width of a quarter of a mile. With the daily variations in the depth of the water portions of the muddy bottom were alternately covered with water and exposed to sun and air. During the very low water in the summer and fall of 1880 coves and detached pockets of water were formed.

In the summer of 1878 intermittent fever began to prevail in the vicinity of this reservoir. The first cases appeared close to the river on the east side at the foot of the mountains bordering the reservoir.

Of a population of eight hundred souls about three hundred and fifty-three were attacked up to October 1, 1882. Up to 1880 cases had appeared in every one of the twenty houses on the east side of the river between the dam and the Pittsfield line. In some houses as many as six cases occurred. The surrounding towns were comparatively free from malaria during these years.

The celebrated lawsuit which followed failed to place the cause of the epidemic upon the company who raised the dam. Looking at the case from the present standpoint we should say that some germ carriers, possibly foreigners, as workmen, wandering gipsies and the like, entered the territory around the reservoir, and the conditions being made very favorable by the drought, the mosquitoes became infected in large numbers. Last autumn I had the good fortune to inspect this interesting region both alone and in company with Dr. Adams. Anopheles larvæ were encountered in the ditches in the meadows, in isolated pools and in the river itself. Those which I reared into adults were all *A. punctipennis*.

I have no doubt that similar severe local epidemics have occurred but they usually fail to find a faithful student and chronicler to put them on record.*

In endeavoring to trace the origin of such localized outbreaks the student of to-day is confronted with the task of searching past records concerning the movement of the population with special reference to the laboring classes. This task, which I have left untouched, would amply repay many a student in searching for subjects for a thesis and it would utilize the experiments which human beings, quite unaware, have been and are still making upon themselves and upon one another, experiments which are of the highest value as they are inflicted upon us in the natural way.

In the position taken that laborers from malarial countries may introduce intermittent fever, I find myself no longer alone, for similar inferences have been made by European observers recently.

The mouths of the Elbe and the Weser river in Germany have been in times past the scene of much work in the construction of wharves, dikes and reclaiming of wet land. Malaria has usually accompanied this work. Martini[53] has recently published an account of several outbreaks which are of special interest to us. A number of Dutch laborers were imported in April, 1901, to work upon a large dike near Wilhelmshafen. Soon after their arrival some were affected with malaria (relapses?). From the scene of their work malaria spread and prevailed in 1901 and 1902.

On the West Frisian coast, similar work was carried on by natives from 1888 to 1900 without the appearance of malaria. In 1899 to 1901, the work was done by Dutch laborers of the company furnishing the party above referred to and malaria appeared. These laborers came from a region in which malaria is endemic. Near the mouth of the

* An epidemic in Deerfield in 1886-1888 is briefly described by Dr. S. W. Abbott in the report of the State Board of Health for 1888.

Ems river malaria appeared during the construction of harbors and a lock on which one hundred Italian workmen were engaged. This subject is considered of so much importance that the government has established a special laboratory at Wilhelmshafen where construction work is being carried on to prevent the outbreak of malaria among the workmen and the inhabitants. Anopheles is abundant in the localities referred to.

Among the outbreaks of malaria which are of more than usual interest are such as occur among the inmates of schools, prisons and institutions of one kind and another. Persons are now and then attacked who have been confined as prisoners.

The few instances which have come under my immediate observation were always near breeding places of Anopheles, and some of the inmates, gathered together from various localities, were evidently old cases of malarial infection or else from places where malaria is endemic. In several cases such persons would reveal their chronic infection by passing through an attack after they had left their home in the endemic territory and settled down in their new abode.

Martini [54] has recently given a very plausible explanation of the early and repeated occurrence of malaria in certain houses. The curves of malaria in the endemic region of North Germany show two maxima; one in April–May, the other in August–September. Last year Martini came to the conclusion, after a study of the local conditions, that the early cases are due to hibernating mosquitoes which are called from their hiding places by the great warmth of the dwellings. They feed upon the blood of persons who have passed through attacks the previous season and find sufficient warmth in the houses to mature and transmit the parasites to other persons in the same dwelling.

There are certain warm days in March and April when

mosquitoes leave their hiding places and sting vigorously whenever the opportunity offers. This year March 14 was one of these enticing days, and I was able to capture a considerable number of females. In a school not far from Boston, situated in a locality favorable to *A. maculipennis*, the first case appeared on April 8. The infection was probably carried by mosquitoes which swarmed March 14, and which infected themselves on that day or later from some latent case dating back to a former season. We should expect to encounter cases about three weeks after such early warm days which allows for the ripening of the sporozoites and the period of incubation of the new case.

It might be supposed that malaria, once established in a house, can pass from one member of a family to another and that cases may be looked for every three weeks during the mosquito season. If this were true, the contagious character would have been recognized long ago. In the foregoing pages, I have endeavored to show why this would not be likely to occur. The patients attacked are treated promptly and gametes cannot form. It may also occur that unusually susceptible persons even, if not treated, do not form gametes for a time. Occasionally, however, the successive illness of members of the same family is noticed. Thus, Dr. Bronson, in the report from which I have already quoted, made the following statement in 1871:

> "The frequency with which the several members of a family, in a previously healthy neighborhood, have sickened one after another when once the complaint was introduced, often suggested to Dr. Barker the old idea of contagion. It must be admitted that authority is opposed to this idea, but I do not think the question yet settled."

In looking over some records which I collected with considerable care about six years ago, in a town then recently infected with malaria as a result of extensive public improvements, I find that house epidemics did not figure to any ex-

tent. In one family, consisting of two adults and five children, the father was taken with quotidian chills, and a child three years old who slept with him was attacked with tertian fever three weeks later. Here there is some evidence of transmission, but it may have been an accidental coincidence.

In a family of six the father was attacked in May, but no other members subsequently. In a family of nine, consisting of the parents and seven children, the father and a child two and one-half years old were attacked in July, within a few days of each other. No other cases occurred later.

In a family of four, only the mother was attacked in July. In a family of five, only the father was attacked in August. In a family of eleven, mostly grown up, two daughters were attacked within a few days of each other, in August. About a month later one other member was taken ill.

A number of other illustrations might be given which would bring out substantially the same fact; large families of whom but one or two, rarely three, are attacked, and usually simultaneously. The houses do not become foci of the disease.

The facts which have been cited to show that houses do not necessarily retain malarial infection, probably because the blood of the diseased members does not produce fully ripe gametes, naturally leads to that very important phase of our subject which relates to the spontaneous disappearance of malaria.

A fact which stands out prominently in the eminently interesting chronicle of New England's ague spots, is the disappearance of this malady after it had prevailed for a time. Tradition represents it very prevalent in the earliest colonial days, but the practitioners of the early quarter of the nineteeth century did not see much of it.

The testimony from various parts of the temperate zone seems to be the same, that places once malarial are now entirely free. Certain portions of England, France, North Germany, and the Netherlands, have passed through periods of malarial endemicity. In such places Anopheles are still present, but the virus has disappeared. Farther south the process may have been the reverse of this. The territory about the mouth of the Tiber, now thoroughly infected, used to be a summer resort for the Romans. The Roman Campagna shows signs of former active cultivation and occupation. In our own State, the epidemics associated with public works have, as a rule, promptly subsided after the work was completed, and in some instances almost completely disappeared.

This transient visitation of intermittent fever may be ascribed, in our climate at least, to several causes, chief among which is the prompt treatment of the disease, and the consequent incapacity of the patient to infect mosquitoes. The general conditions are also quite unfavorable. Among these are the thin population in the rural districts, the scattered homes, the general scarcity of mosquitoes of the genus Anopheles, the occasional cold summers and the seasonal interruption of the propagation of mosquitoes by winter. Where one or more of these conditions unfavorable to transmission are suppressed, as in years of drought, in a dense rural population near stagnant water, or among an illiterate, ignorant class of people among whom cases go untreated, endemicity may reasonably be expected. The longer it is allowed to proceed unchecked, the more difficult its final eradication.

The disappearance of malarial infection from large territories in European countries is probably due to the decrease of wars of invasion, and hence of camp life of soldiers in infected regions, and of infected soldiers from malarial regions in territory hitherto free from infection. The lack

of any systematic use of quinine greatly aided the disease under these conditions.

The spontaneous disappearance of malaria is an indication that it was forcibly and overwhelmingly imported at one time, and that it cannot re-establish itself except under certain new industrial conditions, simulating camp life in many respects, such as the laborers' camps, rural schools, prisons, hospitals and factories.

In the foregoing brief review of the incidence of malaria in our State, I have purposely avoided going into detail concerning local epidemics which I have examined more or less in detail. The reader may readily consult for himself the published documents referred to at the end of this paper, in which the essential data have been brought together.

Though much has been written upon this subject, the student finds great difficulty in utilizing past evidence because there is, as a rule, complete silence concerning the, to us, most important factor, the human carriers of the infection. It is for this reason that I have been induced to step slightly beyond the bounds of the severely critical treatment of evidence in this lecture. My object has been to stimulate physicians throughout our State to collect facts from the new standpoint, for only in this way can the views maintained in this paper be rigorously tested. All biological research of medical problems must finally meet its fate for good or ill by being confronted with the human patient, and this confrontation rests entirely in the hands of the physician.

PROPHYLAXIS.

It would be unfortunate if, after the tendency of our city population to spread into the country had been fully established, malaria should come to fasten itself upon such population and drive it back into the heart of the cities.

There is at present no immediate prospect of such a calamity, yet the increase of rural population in density without adequate improvement in environment might lead gradually to endemicity of malaria.

The number of people in our State, living in an essentially rural environment, is larger than is generally supposed. The twelfth census gives the total population of the State as 2,805,346. In eleven cities, having each a population of over 40,000, the total number of inhabitants is approximately 1,228,000. Of this number, many live in the outskirts in what is practically rural environment. This class may be allowed to offset those living in the heart of the smaller cities, not included in our estimate, leaving about one and one half million inhabitants as country residents. Of the other half, the larger number visit the country more or less and are, to a certain degree, exposed to prevailing malarial influences.

The means to be resorted to, in preventing and suppressing malaria, will, without doubt, occur to those who have read the foregoing pages, and any special suggestions may seem superfluous. Yet, in view of the danger which is always likely to beset the awakening public sense and responsibility of going from one extreme to the other, from ignorance and apathy to a too vigorous, wasteful and often injurious campaign, it seems best briefly to summarize the conditions to be met and the simplest means of attaining the desired end. The two things plainly to be done are to suppress Anopheles as far as possible and to shield the malaria patient so that Anopheles cannot become infected.

In localities where malaria is not known to exist, but where there are breeding places of Anopheles, the destruction of such breeding grounds is not essential from a public health standpoint. If, however, in such or similar localities, public works are to be undertaken, the situation is entirely changed. Great care must be taken to prevent

the introduction of malaria. There should be, at the outset and before any digging or other open air work is undertaken, a thorough biological survey by a competent person of the entire locality which may come under the aerial influence of such work. Breeding places of mosquitoes should be carefully noted and plotted on a suitable map. For this purpose the inspection should be made in the spring, the summer and the fall, as the conditions are likely to be quite different in different seasons. Allowance should be made for drought and the conversion into breeding places of bodies of water free from mosquito larvæ during seasons of ordinary rainfall.

The effect of the work to be undertaken upon drainage, either temporarily or permanently, should be carefully considered. Especially the building of embankments and roadways should be allowed only when their probable effect upon surface water is not favorable to stagnation. Excavations for brickyards, for material for roadways, trolleys, etc., should be forbidden if the ground water level will be exposed thereby and a stagnant pool created. The sins of railroad building, committed in the past, are numerous and practically irremediable. No new ones should be permitted. One of the richest breeding grounds of Anopheles about Boston with which I am acquainted is a triangular piece of marsh completely shut in by railroad embankments and converted into a fresh water pool.[55]

The treatment of existing breeding places, before and during the progress of work by gangs of laborers, may be palliative or radical. The palliative method, now so well known, consists in covering the surface of stagnant, mosquito-breeding bodies of water with some grade of petroleum oil. The oil which is scattered over the surface by pouring, by spreading with a broom dipped into the oil, or by the use of pump and hose with sprinkler, etc., forms a thin film which interferes with the respiration of the larvæ and

kills them. The film disappears by evaporation and must be renewed every two or three weeks, according to conditions.* The amount of light fuel oil required is approximately one ounce to fifteen square feet, according to Howard. According to Kerschbaumer (60, p.119) one square meter requires somewhat more than 0.5 liter of petroleum to completely cover the surface. This would be approximately 21 ounces to 15 square feet. Why he uses so much I am unable to explain. Palliatives in the shape of petroleum or its combinations should not be used upon water unless the larvæ of mosquitoes have actually been found in it. The interference with the normal aquatic life may lead to as yet unrecognized, undesirable results.

A vigorous campaign against all mosquitoes was waged during 1901 and 1902, in the town of Brookline under the local board of health, by Dr. H. L. Chase and described by him recently. This work, done in our midst and adapted to conditions prevailing in our State, may well serve as a model and guide for other towns where a destruction of all mosquitoes is desired.

Wherever the breeding can be suppressed by natural methods as contrasted with the artificial one of poisoning or asphyxiating the larvæ it should be done. In level meadows the open ditches, if more or less continuously filled with water, should be replaced by some form of covered drains. Shallow pools in which fishes cannot subsist should be filled up. Brooks should be kept free from debris washed down or thrown into them. Such debris provides quiet pools where larvæ are usually found if not elsewhere. The larger streams and rivers usually are free but the banks should be searched for detached pools. In the case of very small rivulets, ditches or brooks, some

* Recently a proprietary substance known as Phinotas oil has come into use. It is said to represent some combination of petroleum. It sinks to the bottom when applied and subsequently rises again. It is said to kill all life in waters treated with it.

flushing device may be useful. The water may be dammed for a certain length of time and then discharged all at once. The larvæ will be swept into larger bodies of water and there destroyed.*

It is of interest to observe the want of topographical knowledge of suspicious bodies of water on the part of intelligent inhabitants. We may truly speak of the unexplored wilderness of the average town brook. The land is cultivated to within a certain number of yards of such a brook or stream and an impenetrable tangle of vegetation guards it from intrusion on both sides. Its presence even may be unknown to many who have frequently glanced at the line of bushes and stunted trees under which it is concealed. It has been claimed that mosquitoes will not breed in water thoroughly shaded by trees, shrubs and other vegetation. This may be true to a certain extent, but larvæ are certainly found in shaded pools. In any event, our brooks should be made accessible to inspection even though the vegetation on the banks be largely preserved. Even the greatest care in maintaining the bed of a brook free from obstructions may not avail in times of drought. Pools will form and larvæ will appear in them. Occasional petrolizing or flushing may then be necessary if malaria prevails.

Swamps are usually regarded as fertile sources of mosquitoes, and if actual inspection should prove that the suspicion is true, petrolizing, permanent flooding, or drainage are the only remedies. Not all swampy ground favors mosquitoes however. I have repeatedly been surprised in failing to find a single specimen in them.

The expansion of our systems of parks and reservations and the increasing patronage which these open air spaces are receiving from the public housed in the heart of our

* I found after writing this that Kerschbaumer has also recommended this device of periodic flushing, especially in dry seasons. (60, p. 130.)

cities make it more and more necessary to inspect for mosquito larvæ the bodies of water usually artificial which are scattered over these reservations. Neglect of this precaution might easily convert our parks into centres for distributing malaria to a population otherwise sheltered from it in the confines of the city. Squads of laborers are frequently employed in the reservations and may infect the mosquitoes. I recall one case in a child living near a large park, whose infection could hardly have come from any other source.

Since excavation and other out-of-door work may be undertaken at almost any time in the suburbs of cities, the campaign against the mosquito in such territory should not be allowed to flag, but should be pushed vigorously until all breeding places have been destroyed or at least controlled by sanitary authorities.

The frequent appearance of malaria in the heart of cities at present is in many instances traceable to extensive excavations near by, but considering the summer exodus from our large cities which includes almost every one now-a-days, if only for a half holiday, the sources of urban malaria may be many, and impossible to locate.

In rural communities rain-barrels and open catch basins, which are usually inhabited by Culex larvæ, should also be searched to note any Anopheles larvæ which may have acclimated themselves. Privy vaults have been found to be good breeding places for Culex in regions farther south.

Fortunately for us, salt marshes though very prolific in mosquitoes of the genus Culex do not form breeding places for Anopheles. The immunity from malaria of our sea coast and the banks of estuaries under tidal influence has long been known, and it forms an agreeable confirmation of scientific studies which eliminate Culex as transmitters of malaria.[55] The late Dr. Greenleaf,[56] who had given much attention to intermittent fever about Boston, showed that the estuary of the Charles river as far as Watertown

could not be regarded as a factor in malaria as there were no cases along the riverbank. He also found the park police along the estuary free from malaria.

The extermination of the hosts of mosquitoes, bred in such marshes, does not come within the scope of this paper. It may, however, be said that if attempts made to drain such mashes of the salt water are successful, care must be taken to exclude fresh water from low places in them otherwise Anopheles may move in where Culex was driven out.

During the progress of open air work requiring the employment of large bodies of men who must live in camps, there should be in addition to a thorough control of the breeding places of mosquitoes an equally thorough sanitary supervision of the laborers.

Since much of the infection of mosquitoes and human beings goes on after dark, the camps should not be placed near the centre of population nor near small bodies of water unless the latter can be kept free from larvæ. The laborers should be inspected from time to time, and any suffering from malaria removed to some hospital or other place where they can be adequately protected from mosquitoes. Notification of the disease among the inhabitants should be insisted on at such times and the sale of quinine by druggists reported to the local health authorities. Where the Anopheles mosquitoes cannot be satisfactorily controlled, the laborers' camps should be well screened and the latter forbidden to move about after dark. Whenever the mosquitoes are properly taken care of the personal freedom of the laborers need not be interfered with to this extent.

It would be of interest to study carefully the health of any town where large public works are being carried on with foreign labor but where the workmen scatter after dark to different houses; also where native labor is being employed exclusively.

Where malaria has been prevalent for some years and where it has assumed an endemic character by cropping out under various conditions in the inhabitants, as a result of latent infection the difficulty of completely eradicating the infection is great because the number of carriers of the parasite is large and mosquitoes may infect themselves readily. Under such circumstances, it will generally be found that the water courses, swamps and ponds have been sadly neglected by an indifferent, indolent public. Streams are used as depositories for all kinds of refuse and the conduits are out of repair. Sometimes railroads are to blame for cutting off portions of ponds, lakes, etc., and converting them into stagnant swamps. In some places, improvements undertaken years ago have left the territory more favorable for mosquitoes. In one town I saw an abandoned portion of a river bed favorable for breeding mosquitoes which had been left by the artificial straightening of the course of the river. In almost any town signs of some past favor to mosquitoes, ignorantly bestowed, are found by the interested observer.

Vigorous effort to reduce the mosquitoes coupled with popular instruction concerning personal protection against mosquitoes, prompt treatment of individual cases and the necessity for concerted action, should succeed in every case in a climate like our own. Such success may not be realized for several years because the people can only gradually rid themselves of latent infection.

In all efforts to suppress malaria by the destruction of mosquitoes it should be borne in mind that those breeding places nearest the bulk of the population should be attacked first and then, with increased means and assurance of success, the territory under surveillance should be gradually expanded. Breeding places over half a mile from dwellings are regarded by many observers as not likely to be of much consequence, yet they should be dealt with, if possi-

ble, for they serve as a source for the stocking of nearer territory.

The question naturally arises in our minds in reflecting upon this important subject whether, with increasing density of population, malaria will eventually succeed in fastening itself permanently upon the State in spite of efforts made to prevent and suppress it along the lines indicated. Fortunately no capital spent upon the radical extermination of the mosquito will be wasted. Whatever is done in this direction will redound to the general good of the people. The campaign against the mosquito will lead to a general cleaning up and to the cultivation of waste land; it will acquaint us with our own brooks now overgrown and concealed; it will tend to bring our dispersed and shattered streams back into their natural beds by the removal of unnecessary dams; it will prevent the private transportation interests from riding roughshod over the land, gouging out here and there unsightly holes and leaving them to choke up with water plants. Intensive cultivation will do much to repair the damage done by neglect of arable land. We shall be even more careful about permitting the pollution of our streams than heretofore if, as I believe, it tends to favor the mosquito larvæ.

The success of the struggle against an endemicity of malaria is favored by our climate, which is purified annually by frost. Only a comparatively small number of mosquitoes survive and these must re-infect themselves again in the spring. Even under very favorable conditions it takes a whole summer for them to gather enough momentum to produce an epidemic in the late summer and early fall when the frost again sweeps them away.

On the other hand, there are increasing difficulties to contend with as the growth of population continues. The cosmopolitan character of our immigration and the steady stream of people from malarial foci in Europe tend to

keep up the supply of infection in our midst. The tendency of gathering a fairly dense population in mill-towns, often in the midst of extensive breeding grounds of mosquitoes, makes them compare well in opportunies for infection with the migrating and transitory laborers' camp. Fortunately this "urbs in rure" population at present does not come from malarial districts to any extent, and hence they have been comparatively free from malaria with a few marked exceptions.

We must also bear in mind that we have arrayed nature herself against us in this struggle against a malarial climate. The great fluctuation in the volume of water in our streams from season to season, the resulting tendency toward bringing down large quantities of silt during freshets which gradually choke the currents in the low lands and cause the stream to widen as it grows shallower are all processes in favor of the mosquito. The deforestation of the land feeding these streams is largely to blame for these conditions and, as it continues, who may feel certain that our descendants living in the rich river valleys may not assume the sallow hue of malarial cachexia and drag along a miserable existence for which the greed of their forefathers is mainly to blame? It is safe to predict that there will not be enough physical vigor in these descendants of a highly conventionalized nation to successfully imitate the immunizing capacity of the African native. They will give way to more sturdy immigrants or else the land will be abandoned.

In sketching this future possibility, I do not wish to be discouraging, but simply to stimulate our people because we have still in our grasp the chances for ultimate success.

It is to be hoped that as our control over malaria increases the paralyzing sensitiveness of communities upon this matter may give way to a more active coöperation with the investigator who is endeavoring to elucidate the local

causes of the disease and who is powerless, without the assistance of the public and the medical profession, to make an adequate investigation. The hearty coöperation of the members of the medical profession is of special importance since they meet the disease in its only visible manifestation, the paroxysm in the human subject. In nearly every community some one or more physicians may be found who are prepared to assist and give their time, who are personally interested in the scientific problems involved and their practical value, and who are prepared to meet the slow returns of science by adequate outlay of time and strength. To many of these I am indebted for valuable assistance given ungrudgingly. With the continued coöperation and assistance of these disinterested helpers, medical science may be able to solve the many minor problems which still surround this subject, and public health may be in position soon to deal more firmly and radically with local causes.

SUMMARY.

1. There is no evidence that any but the tertian form of malarial fever can be propagated in Massachusetts.

2. The original source of tertian infection must be looked for in the blood of individuals coming from permanently infected localities in our own country and in southern Europe.

3. There is no definite information as to the stage of disease or relative immunity in which mosquitoes may become infected, but analogy with similar diseases of animal life indicate that the gametes are not formed early in the disease, and that relatively immune persons are the most dangerous especially after fresh exposure because in them gametes form very promptly without causing much or any clinical disturbance.

4. The dissemination of infection is most easily promoted in our latitude when people live in a crowded condition, unprotected from mosquitoes and near breeding places of Anopheles.

5. The decline and disappearance of malaria after importation and epidemic prevalence is in part due to relative isolation of the inhabitants and protection by quinine and from mosquitoes, and in part to the absence of persons partially immunized by long exposure in endemic localities.

6. There is some evidence that sewage pollution in surface waters favors mosquito larvæ directly, through increase in food supply, indirectly, by injuring their enemies.

7. Since malaria may be latent and hence unrecognized, and since it is largely a disease of the lower classes whose movements it is difficult to control, the best method of reaching it is to suppress the mosquitoes whenever possible.

8. Malaria is of sufficient importance to become a notifiable disease.

9. Infants and children exposed should have their blood examined during any disturbance of health.

10. In times of epidemic malaria it may become necessary to make special regulations governing infected persons.

11. It is of great importance that the relation of the widely distributed species, *A. punctipennis*, to the parasite of tertian fever be accurately determined.

12. It is also highly desirable to test the infecting power of fresh cases of tertian fever as compared with relapses and with individuals from malarial countries.

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DESCRIPTION OF PLATES.

(The photomicrographs were prepared by Mr. L. S. Brown, at the clinico-pathological laboratory of the Mass. General Hospital, with the kind permission of the director, Dr. J. H. Wright.) The specimens are all from insects caught in this State.

PLATE I. Fig. 1. *Culex stimulans.* Head and appendages of male. Palpi long, with proboscis between them.

Fig. 2. The same species. Head and appendages of female. Note short palpi and compare antennæ with those of male in preceding figure.

PLATE II. Fig. 3. *Anopheles maculipennis.* Head and appendages of male. The proboscis is in part concealed by one of the palpi accidentally overlying it in the preparation. Note lateral bending of the distal segments of palpi.

Fig. 4. *Anopheles punctipennis.* Head and appendages of female. Note long palpi and compare with short ones of female culex in Fig. 2. Note that antennæ in the females of both genera are alike, but different from those of the males which are also alike. The preparations are mounted in balsam.

PLATE III. Fig. 5. Wing of *Anopheles maculipennis,* showing the spots due to condensation of scales rather vaguely. Mounted in air under coverglass.

Fig. 6. Wing of *Anopheles punctipennis,* showing the yellowish-white spots on various veins. Mounted in balsam.

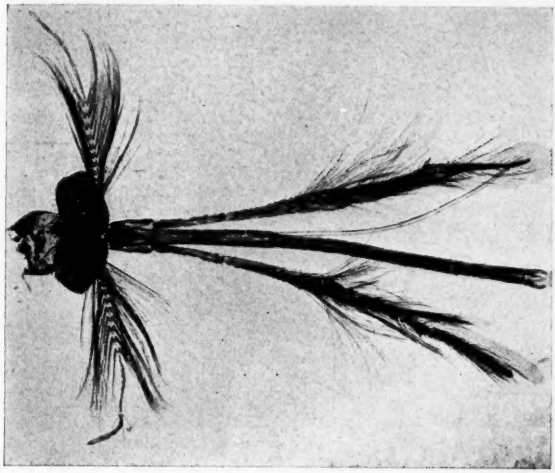

FIG. 1.
Head of *Culex stimulans*. Male.

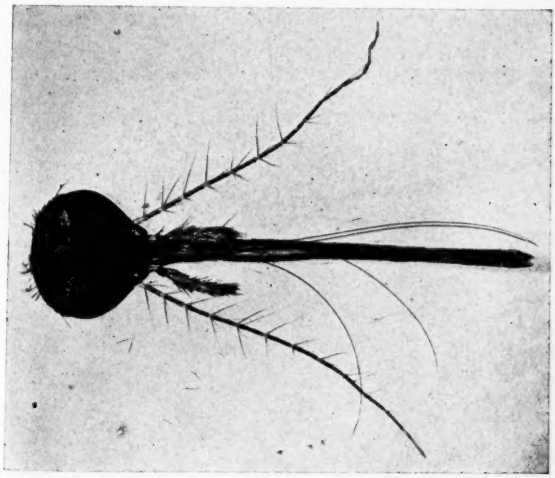

FIG. 2.
Head of *Culex stimulans*. Female

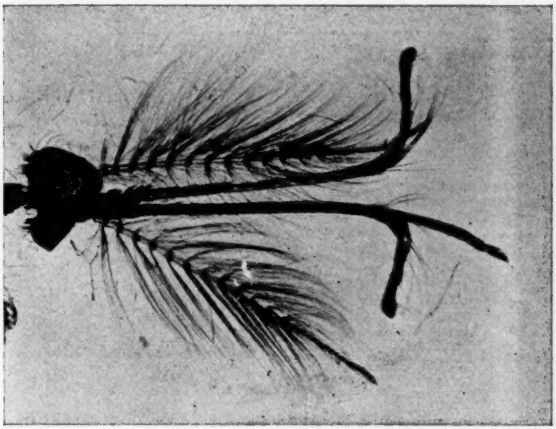

FIG. 3.
Head of *Anopheles maculipennis*. Male.

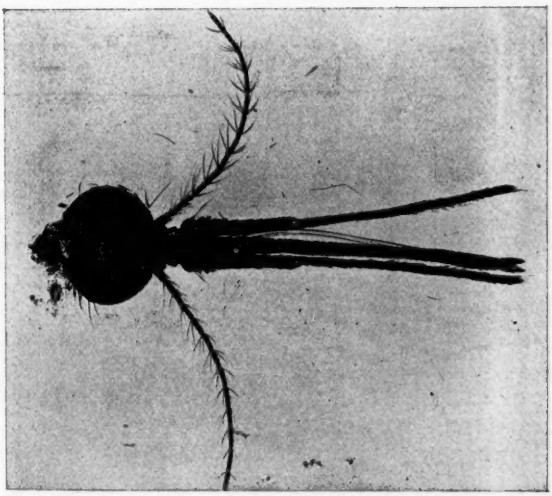

FIG. 4.
Head of *Anopheles punctipennis*. Female.

FIG. 5.
Wing of *Anopheles maculipennis.*

FIG. 6.
Wing of *Anopheles punctipennis.*

ARTICLE XXII.

THE RELATIONS EXISTING BETWEEN RESPIRATORY AND INTRAPELVIC DISEASES.

By DANIEL H. CRAIG, M.D.
OF BOSTON.

READ JUNE 10, 1903.

THE RELATIONS EXISTING BETWEEN RESPIRATORY AND INTRAPELVIC DISEASES.

In 1878 Professor Skene,[9] in his inaugural address, called the attention of the New York Obstetrical Society to the very common occurrence of prolapse of the ovary, and requested the members to devote their energies to the discovery of the causes of this lesion in cases in which the cause was not manifest and in which the prolapse was unassociated with either disease in the ovary itself or uterine displacement. During the ensuing year, Drs. Mundé[12] and Goodell[13] each presented monographs upon the subject, not only considering the causes, but also the course, symptoms, diagnosis and treatment. Various authors have contributed to the subject since and practically all have quoted the list of causes as given in the above papers, and it is because I feel that I can aid a cause which will account for cases not explained by any causes mentioned heretofore that I have undertaken the work of which I trust you will consider this as but a preliminary report.

Clinically, the condition is of immense importance, for Mundé[12] found uncomplicated prolapse in practically ten per cent. of his cases and they are even more frequent now. In the last 500 cases of pelvic disease personally diagnosticated, I have found prolapse of the ovary to play a clinical part as follows:

Total number of cases 500.

Cases in which ovarian prolapse was not a factor . . . . . 296 or 59 %

Cases in which the utertus was retrodisplaced, the ovaries *not* being coincidently prolapsed	62	or 12½%
Cases in which retrodisplacement and ovarian prolapse co-existed	69	or 14 %
Cases in which ovarian prolapse existed with no retrodisplacement of the uterus .	73	or 14½%
	500	100%

The increase of four per cent. in the frequency might be accounted for by the recent greater prevalence of respiratory diseases incident to the annual epidemics of influenza which had not occurred at the time of Mundé's paper in 1879.

My attention was first directed to the relations existing between respiratory and intrapelvic lesions by the fact that patients with pelvic lesions, and especially those with ovarian prolapse, were invariably made worse and their favorable progress under treatment arrested by the intercurrence of any cough-accompanied lesion. In accordance with this conception particular attention was given to the question of antecedent respiratory lesions in all cases in which ovarian prolapse obtained, and especially in those cases specified by Skene in which no cause was manifest. Positive histories were obtained too frequently to allow of its being considered merely coincidental. Very many histories might be adduced to substantiate this statement but I have chosen three which seem sufficient for the present need.

Case I.—M. B. M. Single. Æt. 20. Never pregnant. Never any pelvic infection. For a few months acted as clerk in a drug store. For two years has been "running down." Family history strongly tubercular, but father and mother and one sister living and free from tuberculosis. Three years ago had enlarged glands removed from cervical region. Three small glands now palpable on right. Takes cold easily and always has persistent cough following each cold.

Cough has at times persisted for months until phthisis was feared. About one year ago began to suffer from backache and leucorrhœa, and pain in iliac regions. Catamenia began at thirteen. Regular until recently. Painless. Duration five days until recently; now three or four days. Two napkins a day. No clots. Leucorrhœa variable in amount and not absolutely constant. Exertion increases leucorrhœa which is then thick and yellowish. Has used douches. Not much backache now, but severe at times. Appetite poor. Digestion fair. Bowels fairly regular. Micturition normal.

Physical Examination.—Chest and abdomen negative. Hymen intact but distensible. Nulliparous. Cervix long and conical. Uterus in good position, not enlarged nor tender. Some thickening of the right broad ligament. Left tube and ovary prolapsed and the ovary slightly enlarged [cystic?].

Case II.—B. E. B. Single. Æt. 24. Never pregnant. Family and past history irrelevant. Catamenia began at twelve. Rather irregular, often varying between three and five weeks. In spring of three past years has menstruated every two or three weeks. Painless. Duration six to seven days, using thirty napkins which are well saturated. Clots are frequent, their evacuation being accompanied with pain. Last April (date of history, Sept. 9, 1899) after a severe attack of la grippe in which she narrowly escaped pneumonia she suddenly began to feel pressure in the head, most persistent at the vertex. This vertical pain has been constant since. Worse at times. Often disturbs sleep, pain being worse at night. Can get no specific history. Paternal grandfather lived "a sporty life." Vertigo. Constant leucorrhœa, sufficient at times to demand protection, and of a thick, whitish character. Bowels regular. Micturition normal. General health good except that she is subject to winter cough. Appetite good. Digestion good.

Physical Examination.—Thorax and abdomen negative. Perineum and cervix nulliparous. Uterus slightly enlarged and slightly back of the normal axis and decidedly tender. No discharge from os. Right tube thickened but ovary not felt. Left ovary prolapsed at side of cervix and slightly movable, being apparently loosely adherent.

Case III.—C. S. Married eleven years. Æt. thirty. Has had two children and one miscarriage. Oldest child ten and the youngest three. Miscarriage resulted from a fall. First catamenia at fifteen. Always irregular, but much more regular the past two years. Duration seven to nine days. One to two napkins a day. Severely painful at times necessitating rest in bed. Pain principally in right iliac region. Nearly constant lumbar and sacral backache which is increased by walking and relieved by lying down. Constant but not excessive leucorrhœa. Severe and frequent headaches. Bowels constipated. Micturition normal. Nervous and irritable. General health poor. Has many times feared consumption and has been almost constantly under medical care for pulmonary troubles. Has spent winters in the south to avoid lung troubles. Has raised blood at times.

Physical Examination.—Lungs now clear. Heart normal. Uterus in normal axis and very little enlarged. Perineum good. Cervix lacerated, everted and eroded. Right ovary in the posterior cul-de-sac and enlarged, very tender and firmly adherent. Left ovary enlarged and soft.

These histories were chosen because in them the factors which have heretofore been most frequently assigned a causal rôle, perfectly correctly, are so markedly conspicuous by their absence. And here permit me just a word as to my position. I am not attempting to controvert the work already done on this subject but to amplify the knowledge by the addition of one more etiological factor. Without doubt every one of the causes thus far mentioned,

and especially those of Mundé [12] and the eleven enumerated by Goodell, [13] are all equally or more important.

Goodell, [15] in speaking of the causes of ovarian prolapse summarizes his views in one terse sentence in which he says that any condition which causes a persistent congestion of the ovary should be considered a cause of its prolapse. Authors are unanimous in stating that the most frequent cause is arrest of involution post partum, leaving the broad and ovarian ligaments in a relaxed, inelastic condition. In the above three cases, however, not one of the more active ordinary causal factors is present, and the only one of Goodell's eleven present, is the lacerated cervix which I feel sure was placed in the list mainly for the sake of completeness.

But two of these cases had never been pregnant nor infected and were virginal, showing no evidence of any form of sexual excitation. In the third no sign of subinvolution existed, any enlargement in the uterus being readily accounted for by the cervical and ovarian condition.

Study of the mechanism of cough will show that it most abundantly satisfies the causal requirements, first as to congestion, and secondly, as I hope to show below, as to the force necessary to push the ovary downward. Westbrook [27] has very clearly and concisely described the mechanism of cough, and the following is an abstract of his article:

The muscles of respiration, with the exception of the diaphragm and levatores costarum, are attached to and act upon the upper ribs; the expiratory group make their principal traction upon the lower portion of the thorax. The expiratory group, in which we are particularly interested, are, anteriorly and laterally the triangularis sterni above and the recti, obliqui and transversalis abdominis below. The action of the triangularis is simple and in this connection relatively unimportant. The action of the rectus is also simple. Its sole influence is to draw the sternum

downward, and so to diminish the size of the anterior wall of the abdominal cavity. The action of the obliqui is more complicated; their posterior portions which pass directly from the crest of the ilium to the ribs exert an immediate and powerful downward traction upon the thoracic walls, not only diminishing the size of the lateral walls of the abdomen but acting as adductors to the ribs, which have been put in abduction during inspiration. The more anterior portions have a crossed action and make diagonal traction across the abdomen. The upper portion of the external oblique and the middle portion of the internal oblique of the opposite side, attached as they both are to the intervening sheath of the rectus, are equivalent to one long diagonal muscle, passing from the ribs on one side to the crest of the ilium on the opposite side, and capable by its contraction of drawing the ribs downward and inward so as to contribute much toward the expiratory diminution of the thoracic and abdominal cavities.

The function of the transversalis is equally important. Its middle portion is attached by strong fascia to the transverse processes of the lumbar vertebræ; while the fibres below arise from the iliac crest and outer half of Poupart's ligament. Above it arises from the inner surfaces of the costal cartilages. The fibres of this part of the muscle pass horizontally across the abdomen and by the mutual attachment of those of the opposite sides at the linea alba, one continuous muscle is formed, the contraction of which strongly adducts the lower costal cartilages and thus diminishes the epigastric angle. The middle portion of the transversalis acts directly upon the abdominal wall. In its contraction the transversalis acts from the outer border of the quadratus lumborum as that muscle fixes its tendon in the lumbar region. It tends first to draw the linea alba toward the fixed point; but the antagonism of the opposite side prevents lateral displacement and simply allows the anterior

abdominal wall to approach the vertebral bodies, shortening the antero-posterior diameter. The muscular fibres intermediate between the anterior and posterior attachments make strong lateral compression. The lower fibres when they contract tighten the line across the abdomen and compress the intestines.

The posterior muscles are the erector spinæ, serrati postici and quadratus lumborum. When the erectores spinæ contract they draw the ribs forcibly downward toward the pelvis, lessen the transverse and antero-posterior diameters of the thorax and at the same time tend to shorten the perpendicular axis of the abdomen.

When these powerful muscles of expiration are spasmodically contracted, as in the act of coughing or sneezing, their first effect is to adduct and depress the ribs and by so doing diminish the size of the thorax in all its diameters. But as the abdominal walls only yield in the upward direction, and as the contents of the abdominal cavity are practically incompressible, its roof, the diaphragm, which is relaxed during expiration, must ascend and encroach upon the thoracic space from which air escapes to compensate for the diminution in its size. The second effect of the expiratory contraction is to lessen the circumference of the abdomen. As the contents are incompressible, and as the sphincter muscles, by their contraction, prevent the escape of the contents of the hollow viscera, the decrease in the transverse and antero-posterior diameters must be compensated for by an increase in its long diameter, another factor in the ascent of the diaphragm. Further, during the expiration of cough the glottis is partly closed, thus producing considerable resistance to the egress of the air.

Thus by making a more less rigid walled cylinder of the abdomen, of which the top is to be elevated by internal pressure, the ascent of this top being resisted not only by gravity but also by a partial closure of the glottis, it is

readily understood that the bottom of this cylinder is struck a veritable blow by *contre coup*. Speaking of this effect Westbrook says: "In the pelvis the inferior hæmorrhoidal veins and those of the uterus, vagina and bladder are sometimes so turgid that slight ruptures and hæmorrhages occur, particularly from the uterus, causing a bloody, serous discharge from the vagina during cough. Involuntary urination also sometimes results from the sudden downward pressure, especially in women."

Weiss[76] (Heilkünde, Vienna), as evidencing the expulsive power of this muscular mechanism says: "The expelling speed of the air varies considerably. In quiet respiration it passes through the glottis at 125 cm. per second, and the pressure supports two mm. of mercury. During violent expiration and coughing, the pressure rises to 150 to 200 mm. and the speed is said to exceed that of the wind in a storm. It is said to be as much as 100 meters a second."

Westbrook's article was written apropos of respiratory diseases, but if it had been his desire to defend such a contention as is suggested by the present paper he could hardly have written more directly to the point.

In operating in or through the vagina no one can fail to note the marked rise and fall of the vaginal vault with each respiration, and if perchance the patient cough or vomit the specula are not infrequently forced entirely out of the vagina. This impulse caused by cough is also plainly evident if a patient coughs during a digital examination.

Moreover the effect of respiratory influence has long been recognized in various pelvic lesions, particularly herniæ and uterine displacements, but no mention, so far as I am able to learn, has been made in connection with genuine respiratory diseases. This is true of both monographs and text books, both old and recent. The respiratory influence most frequently invoked is invariably a secondary one in

connection with either the straining due to muscular exertion or that due to intestinal diseases. Almost every author who has written at all comprehensively of displacements of the pelvic viscera has cited straining as a cause. Yet the effect of straining on the pelvic organs is entirely respiratory, being wholly due to the involuntary holding of the breath in inspiration which invariably accompanies the muscular strain. The principal difference existing between the mechanism of strain and that of cough is in the degree of force, and that in straining the glottis is completely closed, with or without the relaxation of the sphincters.

The clinical evidence appearing so conclusive the actual demonstration and measurement of the relation existing between respiratory causes and pelvic effects was all that was lacking. To supply, very crudely it is true, this want, I have constructed this apparatus which, notwithstanding its humbleness, I have christened the pelvio-respirometer.

By its use I have been able to read exactly the force of the downward impact upon the pelvic structures resultant from various respiratory movements. Readings have not been made from a large number of subjects, because, as I expected, I found that the figures varied only within very narrow limits, and the readings given represent the mean average reading with the maximum and minimum extremes only an ounce or a fraction of an ounce above or below.

Before giving the readings, however, I wish to say just a word as to what was to be expected. Many of you have undoubtedly, as I have, seen the so-called "lung testing" machines commonly placed in public places, the dials of which show how many pounds pressure can be created by forcible expiration. All that I have seen are shamefully mendacious. The average expiratory power for an adult man is one and one-half pounds. By training and special gymnastic expansion of the chest a few ounces can be gained. In the adult woman the average expiratory power

is eleven and one-half ounces. The maximum personally noted being fourteen ounces. With this explanation I submit the readings obtained by the use of the pelvio-respirometer.

Downward pressure is produced upon the pelvic viscera as follows:

Ordinary inspiration causes a downward intrapelvic pressure of . . .	1.85 ounces
Ordinary expiration a vacuum of .	7-16 inch
Forced inspiration a pressure of .	2.56 ounces
Forced expiration a vacuum of .	13-16 inch
Slight cough a pressure of . .	7.5 ounces
Violent (artificial) cough a pressure of	16.3 ounces
Moderate straining a pressure of .	6.83 ounces
Strenuous straining a pressure of	10.95 ounces

So that the conditions might as nearly as possible simulate the ordinary conditions, these readings were all made with the patients sitting erect and with the corsets on. One other fact I wish particularly to specify: no readings have been made in cases in which a genuine hard cough due to actual respiratory lesions existed. Much as I desired to do this, and also to measure the pelvic effects of sneezing, I feared to allow my enthusiasm to carry me thus far on account of the possibility of sufficient pressure being brought to bear on the fluid in the vagina to force it through the fimbriated ends of the tubes into the peritoneum. I mention sneezing because but for the trespass upon your time I should have detailed one history in which an attack of hay-fever was the direct exciting cause of the symptoms which were due to an uncomplicated prolapse of the left ovary.

The effect of genuine hard cough of bronchitis, for example, certainly far exceeds that of the counterfeit, and yet I think you will agree that a persistent repetition of such force as has been shown to be engendered in the artificial cough would be quite sufficient to in time drive the ovary down.

Moreover, a study of the topographical anatomy of the pelvic contents shows still further reason for this action of cough. Prolapse of the left ovary is slightly more common than of the right, the reason usually assigned being its greater proneness to congestion owing to its venous return circulation. This reason is undoubtedly active, and is only supplemented by the relations of the sigmoid flexure as described by Deaver.[78] He says: "Normally, owing to its long meso-sigmoid, the greater part of the sigmoid is an occupant of the pelvis. *Here it lies in contact with the bladder, the uterus and its appendages,** and those coils of the small intestines which occupy the pelvis." The attachment of the sigmoid being posterior, if it is in relation with the bladder it must cross the top of the left broad ligament in an antero-posterior direction. Consider what must occur when this sigmoid is driven downward by the effects of coughing, or even straining, especially if this sigmoid is loaded, as so frequently occurs in women. But this downward impact represents only one or two equally important factors. According to Westbrook, the mechanism of cough is such as to exactly supply Goodell's one general predisposing cause, namely congestion.

We have also thus far been considering the effect of cough on normal ovaries. Consider now how much this effect is heightened in the instance of an ovary already predisposed to prolapse by preëxisting lesions. Consider, for example, its effect upon an ovary enlarged, heavy and turgid with the physiological congestion of menstruation, or enlarged and heavy from the presence of a false corpus luteum, or again an ovary but imperfectly sustained by broad and ovarian ligaments weakened by arrested involution, and does not the evidence seem conclusive?

And of all the causes of ovarian prolapse respiratory lesions are certainly amongst the most important. First,

* The italics are my own.

because in our New England climate catarrhal conditions of the respiratory tract, accompanied by more or less persistent cough, are so common as to have by familiarity bred a certain degree of contempt, especially in the laity. Secondly, because this is a cause which does not act insidiously and unsuspectedly, but frankly and openly, thereby offering the greatest possible scope for the highest attainment of medical science; namely, the prevention of disease.

But before proceeding to the consideration of the prevention of complications, I wish to offer one other suggestion. Throughout the literature of the subject I find comparatively frequent reference to congenital ovarian prolapse. My suggestion is that these cases may be better accounted for. In congenital malformations several factors generally act conjointly, and it is anatomically difficult to understand how any such action can produce *downward* and backward congenital ovarian displacement. Ovaries are unquestionably congenitally displaced anteriorly, as in Engelmann's [15] cases, and into hernial sacs, but where such conditions as persistence of the lumbo-ovarian ligament or the existence of the appendiculo-ovarian ligament of Clado obtain, the ovary is maintained above its normal level.

May not, at least a part of, these so-called congenital prolapse cases be due to the congestion and pressure effects of respiratory diseases—for example, whooping-cough; during childhood, no symptoms manifesting themselves until after the maturation of the organs?

If we grant that the above deductions are correct, at least in part, much can be done, especially in women known to be the subjects of pelvic lesions, by prompt and efficient attention to the respiratory lesions; and if for any reason it is inadvisable or impossible to inhibit the cough, much may be done to lessen its evil effects upon the pelvic viscera. And I say pelvic viscera advisedly, for while I

have confined myself to-day entirely to the ovarian conditions you can all easily see the concomitant influence on the other pelvic structures.

The prevention of pelvic complications of respiratory lesions rests not with the gynecologists but with the general practitioner, for it is he who sees the cases long before he needs to refer them to us.

The following suggestions may, with others better known to the general practitioner than to us, serve to avoid such complications.

The congestion of menstruation acting in conjunction with that of cough is always harmful and even the nulliparous should be guarded against it. As corsets lessen decidedly the normal abdominal resiliency and go far toward destroying the so-called retentive power of the abdominal mechanism, they had better be proscribed during any severe and long persisting cough-accompanied lesion, especially during the catamenia.

By requiring a patient to assume the prone position for a few minutes after a violent coughing spell, disengorgement of the pelvic vessels is favored and gravity assists the ovarian supports.

If ovarian, or pelvic, involvement is suspected much good may result from the employment of the pelvic respiratory massage as suggested by Williams. This consists in having the patient assume the genu-pectoral or knee elbow position and aspirate the pelvic vessels by slow forced expiration about fourteen times a minute.

Complications may also be avoided in those already the subject of pelvic disease, by insistently warning them of the necessity of avoiding a cough and the importance of prompt and efficient attention should such a condition arise.

ADDENDUM.

Having pursued this study thus far, which by the way I consider as probably only the beginning, I naturally de-

sired to know if the downward impact of practically one pound, as shown by the pelvio-respirometer, was really sufficient to throw the ovary down. While I do not consider my data as conclusive as yet, and shall investigate the matter further, I have, since the completion of the paper, thanks to the kind co-operation of Professors Thayer, Cushing and Leary, been able to make a series of investigations on the cadaver and learn that in the dead subject a pressure of ten ounces is quite sufficient to carry the fundus of the uterus to the bottom of the cul-de-sac of Douglas. A slightly greater force is requisite to so completely prolapse the ovary. It is carried down to the level of the utero-sacral ligaments by a pressure of twelve and one half ounces, and is completely prolapsed by a pressure of fourteen ounces. The normal vital ligaments would undoubtedly act slightly different from those on the cadaver, but if the respective resistance imparted to the hand is of any value the ligament on the cadaver seemed much more resistant than are those ordinarily felt in operating, but of course totally lacking in resiliency.

Taking these findings as a basis it appears that the force engendered by cough is slightly in excess of that required.

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ARTICLE XXIII.

PROBLEMS OF CLINICAL ANATOMY.

By THOMAS DWIGHT, M.D.
OF NAHANT.

READ JUNE 10, 1903.

PROBLEMS OF CLINICAL ANATOMY.

My attention was called very early in my career, by a murder case in which I figured as an expert, to the fact that all men are not made alike. This started me on my hobby of anatomical variations, concerning which I have had the honor of speaking before this Society more than once. Years of observation have taught me that there is a practical clinical side to these studies. Just as the expert hesitates in estimating the height of a body from a single bone, because he is not aware how much the body in question may have varied from the norm, so the practitioner with his patient before him has often wondered whether some uncommon variation of structure might not be the cause of the impossibility of making the symptoms square with what, according to the books, should be the condition of affairs. There is a wide and but little explored area between the domains of anatomy and pathology; a debatable ground passing alternately under the sway of each of the claimants, which contains very valuable mines of information. It is not pure anatomy nor is it pathology. It treats of conditions which for the most part are not often recognized during life, but the knowledge of which would have helped the practitioner in many a strait. This knowledge, I may add, is of special importance to the surgeon. The purpose of this paper is to point out certain groups of peculiarities which may be of clinical importance. They include not only variations of structures, but also

changes which are the more or less constant results of disease. Increasing knowledge will, I am sure, show us that certain pathological conditions are almost necessarily associated with certain morphological changes. Moreover, the results of certain series of observations are, I think, establishing the law that certain pathological conditions have their origin in some peculiarity of development. Thus certain lesions from trauma occur only when there is an abnormal weakness due to peculiarity of structure. It is worth while to know that certain parts of the skeleton are particularly subject to errors of development, though we may be still ignorant of the cause. Why, for instance, is the last lumbar vertebra the only one in which the arch occasionally, especially in savage races, remains ununited with the body? Or, to take another and more practical instance, for it may effect the motions of the head and be accessible to the X-rays, why are the second and third cervical vertebræ especially likely to be more or less fused, and consequently underdeveloped? Why is the third metatarsal the only one that in the absence of any disease is fused with the tarsal bone supporting it, and why is this fusion limited to the plantar part of the joint? I shall follow none of these topics very far on this occasion, for that calls for minute details which would be out of place to-day. I have, indeed, already written on some of these points and have other papers in the course of preparation and publication. To-day I invite your attention merely to instances illustrating principles. I hope to show that they are of practical importance; nay more, that the deductions from some of them are far-reaching.

As a distinctly clinical point let me call attention to the symptomatology of aneurism of the right subclavian artery. The recurrent laryngeal nerve which winds round it is the chief motor nerve of the right half of the larynx. A certain more or less characteristic paralysis of the muscles of

the right half of the larynx is the necessary consequence of an enlargement of the artery at the place where the nerve loops under it. Nevertheless there may be an aneurism at the very place at which it should give rise to this symptom without its appearing. Why? Because in a certain proportion of cases the right subclavian artery, owing to a deviation from the usual course of development, arises the last of the large branches of the arch of the aorta, and passes behind the œsophagus to its destination. In these cases the inferior laryngeal nerve is not recurrent, but runs from the vagus in one or two bundles directly to the larynx, having no relation whatever to the subclavian artery. I expect to meet with this arrangement at least once a year in the dissecting room. Here is a pretty instance of it.

Let us turn to peculiarities which are the direct results of pathological conditions. Look at these specimens of severe Pott's disease. Note how the aorta is as it were half coiled round the spine, lying in transverse folds with sharp angles. These specimens have been already described in print. The stoppage of the circulation by the compression of the vessel at these angles has been given as an explanation of the sudden death that occasionally occurs in this disease. It should be known that this is the most probable course of the aorta under these circumstances. Here is another and a very different specimen concerning which there might be a good deal of discussion as to whether it is the result of a pathological change or of a congenital malformation. I may not discuss it because an article on it is about to appear in a medical periodical. Let me therefore merely show you this cast of a very remarkable hour-glass stomach which I had the good fortune to find in a hardened body. Plenty of hour-glass stomachs have been shown before now, but the true shape and relations can be learned only when the body has first been hardened. The appearance of this one is, so far as I know, unique.

I leave you to judge whether or not such an observation is of clinical importance.

Orthopædic surgery is, perhaps, the most fruitful field of all for this line of research. Let us think for a moment on all the queer positions of the head. Nothing is more characteristic than its poise. I imagine any portrait painter would say that not only are there certain types of head and face, but that each of these is associated with some peculiarity in the holding of the head. I do not say that structure determines function; for, beyond question, function modifies structure. Though some positions of the head imply structural peculiarities, certain long-continued postures must have their effect on the original structure. This question is full of difficulties; but what I want to insist upon is that a certain carriage of the head, when once it has become thoroughly habitual, predicates a certain antomical condition which it imports the practitioner to understand. Which is the cause of the other, and how much one may react on the other, is not clear. Neither is it easy to say when we cross the dividing line between the peculiar and the pathological.

I have here several specimens which I again pass over lightly as I hope to discuss them more thoroughly hereafter. Look at this fusion of the atlas with the occiput. It is symmetrical; the whole of the atlas is developed, only the condyles and the skull are absolutely fused. Was this congenital or acquired? Evidently such a condition must give rise to a peculiarly stiff carriage of the head with, one would think, limitation of the motions. Here is another specimen in which one lateral half of the atlas is underdeveloped and fused with the skull while the other half stands out free. It is a modern skull which came into my possession only a few years ago. Here is another which would be almost its exact counterpart were it not that the fusion is on the opposite side. It is from a mummy of the

twelfth Egyptian dynasty, and at least four thousand years old. Many years ago I bought in Paris the separated bones of a head. Among them was this occipital which bears on the left a stout process descending from the under side external to the condyle. I was at first at a loss to know what it was, but soon learned that it is known as the *paramastoid process* which is found in many mammals, and is particularly large in ungulates and rodents. It is common enough in man as an insignificant projection, but this one was remarkable for its size and for being apparently fitted to articulate with the transverse process of the atlas. During the past winter I have found it again, and this time can show you the neck. The paramastoid process rests indeed against a corresponding surface of the transverse process of the atlas. The specimen is from a man of sixty-eight, and again it occurs on the left. Now this condition was evidently congenital. It is self-evident that it must have modified the movements of the head. That it involved a peculiar position is highly probable. Who would have suspected such a condition had the man come as a patient? Had the man been X-rayed would there not have been talk of an exostosis or of a tumor? I must beg leave to omit entirely what is perhaps my best case of peculiar conditions at the junction of the head and neck. I hope within a year to publish all these and others in detail and together.

We come now to some of the most practical considerations connected with this subject. X-ray work has brought a new factor into surgery. Its help can hardly be overestimated; but we must beware that it does not sometimes lead us astray. I do not refer here to the ordinary difficulties in the interpretation of the X-ray negative, of the necessity for instance of knowing which side of the object was nearest to the bulb and of allowing for the distance. These and other points are for the expert. I refer to the danger of the misinterpretation of peculiarities of structure. The

specimen just shown is a case in point. It is remarkable how little is known of far more common variations. The late Professor Pfitzner wrote of his astonishment at finding that a distinguished surgeon was utterly unaware that the epiphysis which forms the end of the acromion may remain permanently separate from the rest of the bone. Not only would it suggest a fracture under the X-rays, but it might of course be detached by violence that would not break a normal bone. This naturally suggests the burning question of fracture of the scaphoid bone of the wrist, which I think no one had ever heard of before the X-ray. Now it occurs all the time. My friend Dr. Codman was kind enough a while ago to show me a room full of patients with fractured scaphoids at the Massachusetts General Hospital; and yet I still believe that, putting aside extraordinary cases of injury, a normal scaphoid is never broken, and that the separation into two parts, which undoubtedly occurs, is the result of violence acting on a bone composed of two pieces united merely by cartilage. This also I hope before long to establish.

The late lamented Professor Pfitzner of Strasbourg was the true pioneer in the scientific study of the bones of the wrist and ankle. He declined to be limited to the usually accepted number of bones and declared that there are many other true ones, some of which but rarely appear, while others are almost constant in the early cartilaginous stage. Some lose their identity by absolute degeneration, but more commonly this occurs by fusion with one of the neighboring cartilaginous centres. A case in point is what we commonly call the styloid process of the third metacarpal which projects from the radial side of the base of that bone between the os magnum and the trapezoid in a way that makes the disarticulation of the third metacarpal particularly difficult. Now this styloid process is really a true carpal bone, the styloid, which usually to be sure joins the

metacarpal, but which occasionally joins the os magnum or the trapezoid, thereby considerably modifying the articular lines, or which may indeed persist throughout life as a distinct bone. It is to be noted that such a bone which usually fuses with a certain recognized one, but which exceptionally may fuse with another, is also capable of fusing with both. This accounts for the tendency of particular bones to be fused. At the beginning of this paper I asked why the third metatarsal was particularly likely to be fused with the supporting tarsal bone, and why this always occurs on the plantar side. The answer is: owing to the existence of a bone of this nature deep in the sole of the foot. Both Pfitzner and I myself have seen this several times in feet which present no sign of any pathological process. I have a specimen of it in a beautifully formed foot of a girl of nineteen.

Now it is evident that the persistence of such an occasional bone may give rise to X-ray negatives that are liable to misinterpretation. Many years ago, before the X-rays had been thought of, an excellent anatomist and surgeon thought that he had found in the dissecting room a fracture of the astragalus, because the external tubercle at the back of the astragalus was distinct. This is now known as the *os trigonum*, which may be found in all degrees of fusion, from complete distinctness to absolute incorporation with the astragalus. No one knows how many of these distinct centres of the wrist and ankle there may still be to discover. I had the luck a year or more ago to find a new bone in the ankle (the *os intercuneiforme*), and I think I have found a new one this winter in the wrist. To be sure these discoveries at first may seem of no more value than that of one more asteroid or of a one-horse comet, which nevertheless are telegraphed all over the world; but I am sure that many of them have their very practical significance. Conversely the fusion of bones usually distinct is of practi-

cal importance. I have just mentioned one way in which it occurs, but it is not likely that it is the only one. Fusion of distinct bones is not only a disturbing circumstance to the operator who expects to sweep his knife gracefully between them, but it is of greater importance to the orthopædist who is studying the relations between structure and, let us say, flat-foot. Thus the os calcis occasionally is connected by cartilage or fibro-cartilage with the outer end of the scaphoid. Though I believe the opposite opinion has been maintained, I think from my experience that it is associated with a high instep. What influence it may have on the movements of the foot is a more abstruse problem.

And so on, and so on:

> "Though I'm anything but clever,
> I could talk like this forever,"

and keep on showing you peculiarities which once were (perhaps still are) considered mere curiosities, but which in point of fact are alike of great scientific and of great practical importance. Let no one say that anatomy is exhausted. On the contrary we are just entering on a new era, and on one which concerns especially the practitioner who would be eminent.

ARTICLE XXIV.

RUPTURE OF THE QUADRICEPS EXTENSOR FEMORIS MUSCLE.

By HALBERT G. STETSON, M.D.
OF GREENFIELD.

READ JUNE 9, 1903.

RUPTURE OF THE QUADRICEPS EXTENSOR FEMORIS MUSCLE.

It occasionally happens that the country practitioner runs across something rare or at least unusual in a medical or surgical way. A condition which perhaps he recognizes, but for the description and relief of which he looks in vain in his limited library. He may, perhaps, find some mention of the particular condition with which he is called upon to deal, but for any real practical help or aid he may look in vain. His own library fails him because not sufficiently extensive and he is out of reach of the larger medical libraries of the city. He is then called upon to make use of his own talents and common sense, and his results will depend very largely upon his exercise of those qualifications.

Rupture of the quadriceps extensor femoris muscle is one of these unusual conditions. An accident at least not common, and many surgeons in large practice would, I believe, say rare. At least but little mention is made of the subject in modern text books of surgery, and many of them do not speak of it at all. Agnew, in his Principles and Practice of Surgery, third edition, says he has met with but two cases, and his experience, even at the time he wrote that, was a very extensive one. Connor, in Ashhurst's System of Surgery, speaks in a general way rather briefly of rupture of muscles, but does not go into the details of rupture of any particular muscle or group of muscles. Dennis does not mention the subject at all. The American Text Book of Surgery speaks in a general way of muscular

rupture, as does Da Costa. Wyeth in his Text Book, and Monks in the International Text Book of Surgery, allude particularly to rupture of the quadriceps and to its etiology, symptomatology and treatment. On the whole, the knowledge to be gained by the general practitioner from those text books most likely to be in his library would be rather small.

As I have said, rupture of the quadriceps extensor muscle is not a common condition. It is most often of traumatic origin. That is, the patient in going down stairs, or down a steep incline, loses his footing and in his effort to save himself strongly contracts his quadriceps, which was previously very firmly contracted, with the result that it ruptures, or the patella is broken; and the determining factor may be the comparative weakness of the muscle or tendon. Konig, quoted by Klein, describes two ways in which this rupture may be produced. Either the actively contracted quadriceps is suddenly strongly stretched, as when one descending a mountain slips and falls backward; or the passively stretched muscle undergoes an active contraction as when one falls over a step. The weight of the body presses the thigh against the leg, forming a very acute angle at the knee joint. The stretched quadriceps contracts to avoid overstretching, and a ruptured quadriceps tendon or fracture of the patella results. Cases of rupture of the quadriceps, as of other ruptures of the body, have occurred following typhoid fever, and other prolonged pyrexias, from slight strain only, and without the knowledge of the patient; but of these I shall say nothing. Certain factors, however, have at times seemed to play a part in producing weakness of the muscle fibre, rendering the rupture easier than would seem to be normal. Alcoholism and arterio-sclerosis would naturally be looked upon as such factors. Evidences of a diseased condition of the muscle itself has seldom been found. Maydl reports fatty degeneration of the muscle

in one case. Hafeman found tabes dorsalis in two cases. Kohl and Wunsch saw cartilaginous bodies imbedded in the tendon. Kohl found beside three solid masses in the tendon; also an analagous body free in the joint, and he considered the cause to be a chronic athritis. Wunsch found a cartilaginous formation on both sides and diagnosed with probability an exostosis of the tendon. In a case reported by Vulpius there was very marked fatty degeneration of the tendon confirmed by miscrocopical examination; but these were only single and unusual cases. Most of the cases examined show no pathological or degenerate change whatever.

It seems to be most frequently met with in males. In ninety-eight cases collected by Walker, ninety-one were males, and seven females. This great preponderance is no doubt due to the greater muscular activity of life followed by the male sex. It occurs slightly more frequently as age advances, the majority of the cases reported being over fifty years of age.

Concerning the location, there seem to be no factors which tend to make one limb any more liable to this rupture than the other. It seems to occur as frequently upon one side as upon the other. Cases have been reported where both muscles have been ruptured simultaneously; and both muscles have been ruptured separately, an interval of several months or several years intervening.

The point of rupture is most apt to be at the upper margin of the patella, although it often occurs higher up. In its relations, this muscle lies immediately over the joint cavity of the knee, and when the rupture occurs at the upper margin of the patella, the joint capsule is very apt to be opened. Walker says that the joint capsule extends upwards behind the rectus tendon for a distance of five centimetres; and that rupture of the muscle at any point within this distance is very liable to involve the joint.

The occurrence of rupture is usually indicated by a sudden sharp, stinging pain, accompanied by a snapping sensation as of something giving way, and associated with complete loss of function if the rupture be complete; if not complete, if the lateral portions are still intact, or if a portion of the fascia is still intact, some power of extension will still remain. If seen soon after injury, the deformity is characteristic. The patient is unable to extend the leg, the upper edge of the patella is felt and is freely movable in every direction except upward; there is a marked depression above in which the condyles of the femur can be plainly felt. Above this depression is a tumor caused by the retracted muscle. There is but little pain usually, and only moderate tenderness. There may or may not be, coming on early or late, discoloration of the skin produced by extravasated blood. Extension, and flexion of the leg to an angle of 45°, cause little pain. Seen later, that is from one to fourteen days after the injury, the gap so noticeable above the patella at the time of the traumatism has become filled in with extravasated blood and is much less noticeable.

The diagnosis should not be difficult in cases seen early. The loss of the power of extension of the leg, patella easily made out and movable in all directions except upward, a depression admitting one or more fingers above the patella; these objective symptoms, together with a history of muscular effort and the subjective symptoms of sudden pain and a sensation as if something had snapped would, seemingly, make the diagnosis easy. McCosh believes, however, from a study of the reported histories of such cases, that in many of them the injury was overlooked because the examining physician was not alive to the possibility of such an accident.

And now comes the difficult problem of all to solve. How shall this injury be treated? Shall we rely upon mechanical means to bring about coaptation with the hope of a firm, fibrous, union and a nearly functionally perfect leg? Or shall

we invite the dangers attendant upon surgical manipulation of the knee joint, and cut down upon the ruptured muscle, bring the ends into opposition and stitch them with some suitable material?

Other things being equal, the operative method would seem to be the preferable method in the hands of the skilled surgeon and under favorable surroundings. Maydl in fifty-six cases of complete rupture of the quadriceps, states the results in forty as follows: In twenty-four, or sixty per cent., complete recovery followed. Nine of the remainder acquired a useful leg. The other seven were unsatisfactory. Walker states that of twenty-three complete ruptures of the quadriceps in which he was able to give results, in sixteen, or seventy per cent., complete recovery followed. In five a useful leg was obtained, and two cases were unsatisfactory. Compare this report with Walker's report of his operative cases, twenty-one cases so treated and nineteen, or ninety per cent., complete recoveries.

Walker also compares the time occupied in bringing about complete recovery by the two methods. In speaking of his series of twenty-three cases of complete rupture, treated by the mechanical method, sixteen, or seventy per cent. made a complete recovery. Of these sixteen cases, eighteen per cent. recovered in three months, thirty-one per cent. in six months, and fifty per cent. in one year. Of his series of twenty-one cases treated by the operative method, nineteen or ninety per cent. made complete recovery. Of these nineteen cases, fifty-six per cent. recovered in three months, and one hundred per cent. or the total number recovered in six months, showing a large saving of time to the patient by the operative method in skilled hands.

Unfortunately, however, rupture of the quadriceps extensor muscle does not always occur in the young, strong, vigorous adult, the kind to whom you look for favorable operative results. I have before spoken of the fact that a large

number of these cases occur after the age of fifty years. From that age on we look for degenerative changes—arterio-sclerosis and its very common accompaniments, tense, wiry, blood-vessels, cirrhotic kidneys, enlarged prostate,—to say nothing of the greater liability supposedly, of failure of union of the approximated ends.

Again, it is not always possible to bring about a meeting between the patient suffering from the injury under discussion, and the skilled surgeon.

Under these circumstances, it would seem to be perfectly proper treatment for the general practitioner to at least try the effect of the mechanical method of treatment. Many of us would certainly hesitate to molest the knee joint unnecessarily. McCosh, who cannot in any sense be accused of favoring the mechanical method of treatment, in his paper upon this subject, advises against operative interference unless conditions are most favorable, believing that the non-operative plan of treatment should be tried, at least until more favorable conditions can be obtained. And he further says that a delay of several weeks does not contra-indicate the operation. I believe there is one danger from non-operative treatment which must be thought of, and which might become troublesome unless guarded against, that is, inactivity atrophy of the quadriceps. But early massage and passive motion would probably, to some extent, delay if not prevent this change.

All agree that in cases of partial rupture in which the lateral portions of the muscle are still intact, the mechanical treatment is all that is necessary. Most men, I believe, are in favor of mechanical treatment in cases where the distance between the divided ends is not more than one inch, and where the joint has not been opened. In those cases where the distance between the divided end is more than one inch, or where the joint has been opened, operation would seem to be the proper course to pursue; but

even here the general condition of the patient would be a large factor in finally determining upon the proper plan of treatment. And in those advanced in years and presenting the changes incident to age, even there the mechanical method of treatment might be thought advisable.

Particular care should be given to the application of the mechanical method. The patella should be pulled up as far as possible, and held in place by adhesive strips. The leg should be bandaged from the ankle to the patella, care being taken to have the bandage reinforce the plaster strip as far as possible. Beginning above at the upper portion of the thigh, bandage downward firmly, the object being to push down the retracted end of the muscle into nearer apposition with the patella. A long, posterior splint extending from the ankle to the gluteal fold should be applied and the leg strapped firmly to it. Cold applications to the knee if there is much extravasation of blood, foot elevated about six inches above the level of the pelvis. After five or six weeks, massage. After eight weeks, passive motion of joint and crutches. The time of recovery based upon reported cases should be expected in from three to twelve months.

The operative method consists in cutting down upon the separated ends and uniting them, either with silk worm gut, cat gut, kangaroo tendon or silver wire. If insufficient tendon remain attached to the patella, then drilling through the upper portion of the patella and attaching the upper fragment to the patella. Elevated position of the foot for two or three weeks, passive motion, etc., as with mechanical treatment.

Walker concludes his admirable paper with six conclusions, four of which would seem to be most useful suggestions for the general practitioner in deciding upon the proper method of treatment for this particular injury.

"First. In recent cases in which there is not much

effusion, where the joint is apparently not opened, in which the separated ends can be approximated and retained by suitably adjusted pads, the mechanical treatment may be favorably considered. In the hands of an intelligent practitioner, this method may be expected to bring about a complete recovery in the larger number of cases. From nine to twelve months will be required to reëstablish fully normal functions.

Third. The skilled aseptic surgeon who promptly resorts to the operative method in suitable cases, though age and vitality of each patient must be most carefully considered, may quite reasonably hope to obtain a better result in the larger number of patients, and save his patient three to six months' time.

Fifth. Where the separation is greater than one and a half inches, or where the case has not recovered under mechanical treatment, the operative is indicated.

Sixth. As the length of time required for treatment is a very important consideration, so the operative method which has diminished this period and also succeeded in a large number of cases without increasing the danger, will be more often indicated and more frequently applied in the hands of the skilled surgeon."

My attention was particularly called to this condition, by the following case: S. B. S., retired, seventy-one, married, never ill. On September 7, 1902, in going down stairs his slipper caught on the stair and he was thrown forward. He made a strong effort to save himself, and felt something give at his knee. Upon getting up he was unable to stand upon the leg, although the effort did not cause severe pain. The knee was "wobbly," as he expressed it, and uncertain; slight swelling occurred at once about the knee. Seen one-half hour after the injury. Left knee somewhat swollen; swelling not confined to the synovial cavity. Patella drawn downward about half an inch. Immediately above

the patella is a depression allowing two fingers to be laid in; condyles of the femur plainly felt; above the depression is a moderate enlargement of apparently muscular tissue. Flexion, extension, latero-flexion, not accompanied by much discomfort. Patient can flex, abduct, and adduct, but cannot extend the leg. Treatment, posterior splint applied, with a compress of lead and opium wash, moderately firm bandage. On the 8th, the patient was seen in consultation by Doctors Twitchell and Zabriskie of Greenfield in reference to the advisability of adopting the operative plan of treatment. In view of the fact that the patient was the victim of some arterio-sclerosis, and had for some time had some urinary retention due to an enlarged prostate, it was deemed best to try the mechanical treatment for a time, having in view the operative plan of treatment at some future time, if satisfactory results were not obtained by the mechanical. The patient was allowed up in a wheel chair. The leg was kept in splints until the last of October, when massage and passive motion were begun. On November 1st, the patient was walking about with the posterior splint still on. Without the splint he can bend the knee and has some power of extension. Ordered continuance of passive motion. On the 28th of January, 1903, examination shows extension to be strong and forcible. Is able to kick vigorously; motion is good and unimpaired. As compared with the opposite leg, it would seem that he had from seventy-five to eighty per cent. of normal function. Patient says that he can see improvement still going on. Patient shows no lameness, climbs in and out of carriage easily and attends to his usual duties apparently as easily as before the accident.

DISCUSSION.

Dr. S. J. Mixter, of Boston: I should be happy to say something on this very interesting paper, but the fact is I have never had a case of rupture of the quadriceps extensor; that is, a complete one or anything near complete. I have had partial ruptures. I have seen several ruptures in the practice of other men. None of these have been operated on; they have been treated mechanically with good results. A distinguished member of The Massachusetts Medical Society had such an accident at the time I was a student. He was treated by Dr. Bigelow, and got a very good leg. I think I am correct in saying Dr. Bowditch had that accident by jumping from a horse-car. At that time of course operative procedures were not so common as now and on account of his age, at any rate, it was best to follow a conservative line of treatment. He made an excellent recovery. Slight ruptures I have seen. It seems to me that ruptures of the quadriceps extensor tendon are very like fractures of the patella. In certain cases of fractured patella we get excellent results without operation. Those are cases in which the fascia is not widely torn, where the fragments are not separated. It is not the fracture of the patella so much as tearing of the fascia which causes trouble, and if we have only slight separation of fragments either of the patella, which is only a sesamoid bone in the course of the tendon, or the tendon itself, mechanical means will be able to give good functional results.

Dr. H. Warren White, of Roxbury: This paper is very interesting to me just now, after having taken care of a case similar in many ways. A man of seventy in going down steps thought there was another step. He made an attempted step and recovered himself, but felt, as he described it, as if he "had torn his pants." There was a giving way of something, he looked down and found he could not manipulate his leg, could not extend it at all. He was completely incapacitated and brought home and treated with plaster-of-Paris bandages. But the curious part of our case was, that at the end of ten weeks, after re-

moval of the plaster-of-Paris bandage and what appeared to be a complete and satisfactory result, and wearing an ordinary rubber bandage, he was in the stable attending to some duties when with a slippery floor under him his foot gave way suddenly and the same accident happened again, absolutely the same as far as the gap was concerned; perhaps not quite as extensive, that is, the gap was not quite so wide as at the first time, but there was the same inability to extend the leg. He is at the present time lying in bed with a plaster-of-Paris bandage on and we are hoping of course for as good a union as before. I had a consultation in this case and the same conclusion against operation was arrived at, that Dr. Stetson speaks of. Our patient was old. He also had urinary trouble and there was the slight arterio-sclerosis of which he speaks, and signs of the same muscular weakening or degeneration which probably permits such an accident to occur in these cases.